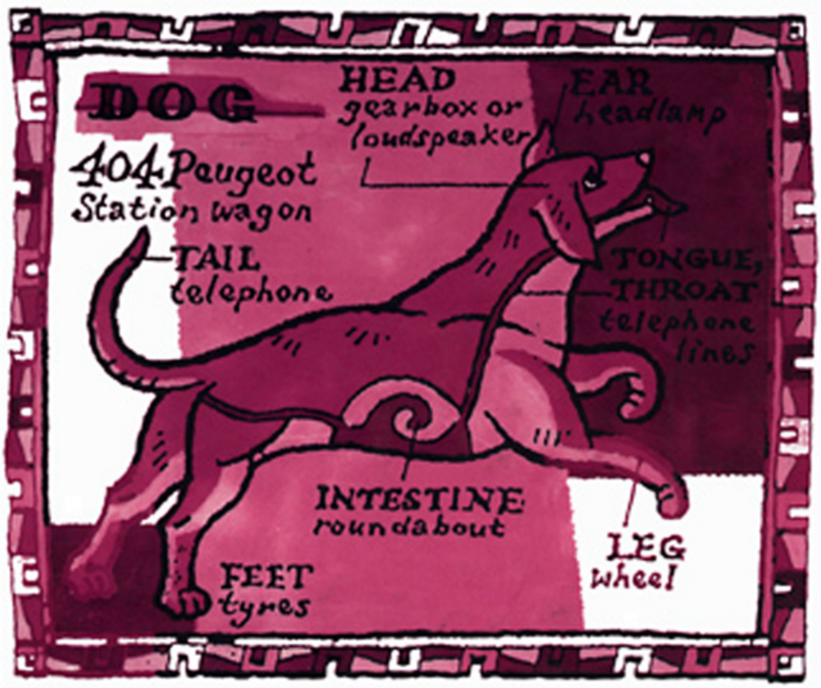
THEORY IN ARCHAEOLOGY

A world perspective



edited by

PETER J. UCKO

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GENERAL EDITOR'S PREFACE

Why does the world need archaeological theory? The purpose of the Theoretical Archaeology Group series is to answer the question by showing that archaeology contributes little to our understanding if it does not explore the theories that give meaning to the past. The last decade has seen some major developments in world archaeology and the *One World Archaeology* series provides a thematic showcase for the current scale of enquiry and variety of archaeological interests. The development of a Theoretical Archaeology series complements these thematic concerns and, by focusing attention on theory in all its many guises, points the way to future long-term developments in the subject.

In 1992 the annual Theoretical Group (TAG) conference was held in Southampton. Europe and the world of archeological theory was our theoretical theme at this EuroTAG conference. We stressed two elements in the structure of the three-day conference. In the first place 1992 had for long been heralded as the time when the single market would come into existence combined with moves towards greater European unity. While these orderly developments could be planned for the sessions organized around the role of archaeology and the past in the construction of European identity, no one could have predicted the horror of what would occur in former Yugoslavia. Throughout 1992 and beyond, the ideologies of integration and fragmentation, federalism and nationalism vied with each other to use the resources of the past in vastly different ways.

The second element recognized that 1992 was a notable anniversary for theoretical archaeology. Thirty years before Lewis Binford had published his first seminal paper *Archaeology as Anthropology,* in *American Antiquity*. This short paper was a theoretical beacon in an otherwise heavily factual archaeological world. From such beginnings came the influential processual movement which, in its early years, was referred to as the New Archaeology. Thirty years has clearly knocked the shine off such bright new futures. In the meantime archaeological theory had healthily fragmented while expanding into many areas of investigation previously regarded as off-limits to archaeologists and their mute data. Processualism had been countered by post-processualism to either the enrichment or irritation of, by now, partisan theoretical practitioners. EuroTAG marked the anniversary with a debate involving the views of Lewis Binford, Chris Tilley, John Barrett and Colin Renfrew, supplemented by opinions from

the floor. Their brief was to outline the theoretical challenges now set before the subject. The audience heard various programmes of where we might go as well as fears about an uncertain theoretical future. Both optimism and pessimism for another thirty years of theoretical excitement were to be found in almost equal measure. However, the clear impression, exemplified by the number of people, almost 800, who attended EuroTAG was that the strength of any future theoretical archaeology now lies in its diversity.

How different in numbers attending and diversity of viewpoints from the early days of TAG, an organization whose aims have always been simple: to raise the profile of discussion about the theories of the past. The need for such a group was recognized at the first open meeting held in Sheffield in 1979 where the programme notes declared that 'British archaeologists have never possessed a forum for the discussion of theoretical issues. Conferences which address wider themes come and go but all too frequently the discussion of ideas is blanketed by the presentation of fact.' TAG set out to correct this balance and achieved it through an accent on discussion, a willingness to hear new ideas, often from people just beginning their theoretical careers.

EuroTAG presented some of the influences which must now contribute to the growth of theory in archaeology as the discipline assumes a central position in the dialogues of the humanities. As expected there was strong participation from European colleagues in sessions which focused on Iberia and Scandinavia as well as discussion of the regional traditions of theoretical and archaeological research in the continent, an archaeological perspective on the identity of Europe and multicultural societies in European prehistory. Set beside these were sessions devoted to visual information, food, evolutionary theory, architecture and structured deposition. Two archaeological periods expressed their new-found theoretical feet. Historical archaeology argued for an escape from its subordination to history while classical archaeology embraced theory and applied it to its rich data. Finally, the current issues of value and management in archaeology were subjected to a critical examination from theoretical perspective.

Nowhere was the polyphony of theoretical voices, issues and debates more clearly heard than in the session devoted to world perspectives on European archaeological theory. While EuroTAG was a moment to reflect on the European traditions and uses of theory, a comparative view was needed if such concerns were to avoid the call of parochialism. Here at the heart of EuroTAG was an opportunity to see the debate in action—not as the preserve of individuals but as a dynamic answer to the question, Why does the world need archaeological theory?

ACKNOWLEDGEMENTS

The EuroTAG organising committee consisted of Clive Gamble, Sara Champion, Simon Keay and Tim Champion. They were helped by many staff

and students from the Department of Archaeology at the University of Southampton and particularly by Cressida Fforde and Olivia Forge who did the lion's share of the organization in the final days. Thanks were also due to Peter Philips who videotaped the debate and to Mike Corbishley, Peter Stone and Eric Maddern who organized videos and storytelling while the art of Carolyn Trant and Sylvia Hays provided the art exhibition. Financial support came from The Prehistoric Society, the TAG travel fund, Oxbow books, Routledge and the University of Southampton.

Clive Gamble
July 1994

PREFACE

This book derives from a conference—flagged as EuroTAG—of the Theoretical Archaeology Group, held at Southampton University from the 14–16 December 1992.

While I had been away somewhere, my colleagues in the Department of Archaeology at the University of Southampton had rather surprisingly chosen to highlight the European political dimension of 1992. In view of the Department's traditional commitment to a world archaeological approach in all its teaching and research, I then offered a day-long symposium entitled 'A World Perspective on European Archaeological Theory' to the EuroTAG organizing committee. My intention was to make it clear that Europe was not only a concept constructed from within, but was also 'seen' from outside.

The 'World Day', as it became known, was a success. A member of the audience wrote later: 'I particularly liked the World session at TAG—several of the papers were an eyeopener—by far the best session that I attended at the conference' (and see Paddayya 1993 for a speaker's view-point).

Yet, in actual fact, the day's meeting served to demonstrate that my original set of assumptions in constructing the programme had been falsely based. Naively I had assumed that it was likely that the theoretical archaeology of those countries which had been under particular colonial and language regimes would reflect such domination:

> How far—if at all—have the world's archaeologies been influenced by *European* archaeological theory? Has such influence been tied to the particular archaeological theory of the European period of the countries concerned? In parts of the world which have had no European periods, what kind of alternative archaeological developments have occurred? Each speaker will pay attention to any theoretical developments deriving from the specific conditions of the practice of archaeology of their particular country or region. Each will also explore why theoretical archaeology has —or has not—'taken off' in their areas of the world. Where applicable, discussion of each presentation—or set of presentations—will be led by one or more European discussants from the country or region responsible for the European period concerned.

The programme started with the United Kingdom—followed by English-speaking India and West Africa, with subsequent short presentations from Australia and Canada; then came Brazil, with a comment from Portugal; Indonesia followed—but without the subsequent planned perspective from Holland (the Dutch declining to speak); a presentation on Namibian archaeology was discussed in the context of German archaeology; and Madagascar was followed by a short contribution on French archaeological theory. Somewhat hesitatingly I had then programmed Japanese archaeology to be followed by archaeology in the former Soviet Union, on the curious, and ignorant, assumption that one or both might originally have had archaeological traditions independent of western European influence. Latin American archaeology was succeeded by a contribution from Spain; China was again followed by a Russian comment and, finally—and, possibly with reference back to the United Kingdom—Ireland and South Africa.

The 'World Day' involved a steep learning curve—the 'independent' archaeological traditions of China and Japan and the former Soviet Union had been nothing of the sort; Brazil had more or less ignored Portugal, and likewise Latin America had largely ignored Spain! Everywhere, the archaeological theories deriving from the United States simply could not be ignored. In addition, everything about the *origins* of archaeology was much, much more complex than I had ever imagined (Sparkes & Ucko in preparation). Nevertheless, by 12 January 1993, it had been decided that the present book should be prepared under my editorship.

During 1993 the nature of the 'World Day' publication venture changed significantly. Some of the changes resulted from the fact that authors from Madagascar, Spain and China had been unable to produce revised papers along the lines suggested by me. Other changes derived from my conviction that this book would have to include some of the details of the European archaeological theory which some extra-European countries had—and others, had not—chosen to adopt. Meanwhile, the organizing committee had decided not to produce a publication deriving from the EuroTAG symposium on 'The Regional Traditions of Theoretical and Archaeological Research in Europe'. As a result, several of the authors who gave papers to the latter symposium have rewritten and reorientated their original contributions to fit into the present book. All of the contributions to the original 'World Day' which are included in the following pages have been rewritten and enlarged to conform to the book's new aims. Chapter 11 is a revised version of an article first published in Catalan in 1993 in *Cota Zero, Revista d'Arqueologia i Ciènca, Vic (Spain)* 9, 102–9.

In some strange way, therefore, this book has created itself from events which were not particularly of my choosing. Yet, I believe that the editorial reorientation of several of the contributions in this book has produced a set of strikingly interesting papers which—taken together—open up a quite new vista on the relationship of theoretical archaeology to its practice in different historical, political, cultural and economic contexts.

I have a large number of people to thank for making this exciting and, I believe, important book possible.

First, at the conference itself: all those participants who joined in discussion, and all those speakers who saw—at least as a result of participating in the 'World Day'—what was intended, and subsequently so thoroughly revised their papers for publication; Professor Henry Wright who presented, and enlarged on, the Madagascan contribution in the unavoidable absence of the Malagasy author; Margarita Díaz-Andreu for verbally commenting on the Latin American contribution from a Spanish perspective; Sara Champion for having insisted that I see Ireland through colonialist archaeological eyes; Paul Sinclair for having concluded the 'World Day' sessions; Sir Gordon Higginson, Vice-Chancellor of the University of Southampton, for his hospitality; and Cressida Fforde and Olivia Forge for having kept everyone so happy in their uniquely informal manner…

Second, in terms of the book: Clive Gamble for having given me a free hand, and the wisdom of his comments on what I was trying to achieve with this book and for showing how to include Bassey Andah's chapter in it; Katharine Judelson for editorial work beyond the call of duty; Sara Champion for assistance in finding Irish illustrations; Susanne Diamond for providing photos from South Africa; and Jane Hubert for having struggled for many hours to make each chapter (and, in particular, my own contribution) as comprehensible as possible.

P.J.Ucko
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INTRODUCTION
ARCHAEOLOGICAL INTERPRETATION IN A WORLD CONTEXT

PETER J.UCKO

Most archaeologists would probably agree that all archaeological classification—and any other form of archaeological activity—must, by definition, be based on some theoretical preconception or other (e.g., Hodder 1991b:7; Sherratt 1993: 123). At any other level of analysis, however, there would be little agreement about the relationship of archaeological theory to practice, nor even, perhaps, about what constitutes 'archaeological theory'. In some cases, existing difficulties of mutual comprehension are due to disciplinary assumptions. Thus, Bernal (1994:119) has recently claimed that 'It is widely believed that "classics" is the academic discipline furthest away from modern politics. It is not merely supposed to inhabit the ivory tower but to be in its topmost storey' Whereas this correctly reflects the image that prehistorians in the United Kingdom have of their Classicist colleagues, in Brazil (Funari, Ch. 10, this volume) the opposite is true: the Classicist is—by virtue of a knowledge of foreign languages and travel overseas—assumed to be the most likely to 'indulge in' (subversive?) theorizing.

Over the past decade or so, the realization has grown that all study and interpretation of the past—whatever the particular interpretive framework—is undertaken within a socio-political context which itself moulds the nature of the interpretation which is to be offered:

> Social constructions of both the past and the present are pliable, flexible and amenable to different interpretations and interests. Anthropologists [including archaeologists] and historians are master builders and, as a consequence, their roles in the complex fields of domination and subjugation need careful investigation.
>
> (Bond & Gilliam 1994:5)

It is clear, therefore, that recognition of the particular theoretical frame-work within which archaeologists choose to operate will be of the utmost significance. Despite this, most archaeological practitioners, of whatever kind, are not able to formulate their own theoretical orientations themselves, let alone place any such theoretical orientation within its historical context (Sherratt 1993:127). As this book reveals, the majority of archaeologists simply perceive archaeological theory 'as separate from and not an integral part of practice' (Cooney, Ch. 12;

and cf. Paddayya, Ch. 6). Following Bond & Gilliam (above), it can be argued that self-consciously aware archaeologists need to force their colleagues to change their attitudes and to recognize the importance of archaeological theory. They should seek to remedy the fact that, as Thomas claims (Ch. 17; and see Hodder 1991b:8), theoretically inclined archaeologists are in the minority in all countries, and are thus forced to form a ghetto within their discipline.

THE NATURE AND IMPORTANCE OF ALL KINDS OF CONTACTS

Archaeology, at least where it is archaeology of the kind which is characterized by the analysis of past material cultures rather than by the study of written records, is usually recognized as an introduction from western Europe, supposedly deriving (according to most authorities) via seventeenth—and eighteenth-century antiquarianism (but see Sparkes & Ucko in preparation) and then from European socio-political and cultural events in the nineteenth century. In so claiming, many variations which may exist within archaeological enquiry are often ignored. Some of these differences may have developed through the very process of 'export' (and see Said 1983:226–42). Thus, the particular moment of its reception in any particular country has often coloured the way that archaeology came to be practised—and thought about—in its new context. In Canada (Mackie, Ch. 8), for example, archaeology arrived at a time when anthropology was in the grip of functionalist (static) theory; for this reason, archaeology became definitively a part of Canadian historical (and not anthropological) enquiry, with all the interpretative ramifications which this has entailed. On the other hand, although the importing of archaeology into China obviously took place in quite different circumstances, yet there too its acceptance as a legitimate and favoured method of enquiry was in the context of historical enquiry—as *substantiator* of written historical accounts (Falkenhausen 1993:842).

An assumption of uniformity underlying archaeological enquiry anywhere in the world is belied by such different historical developmental contexts in different places. Different archaeological traditions (for example, those of the United Kingdom and the former Soviet Union—Trigger 1989:207–43; Kohl 1993:18; Klejn 1994) may seem more similar than they really are. Such apparently extraneous factors as traditional conventions of national archaeological funding practice may have profound effects on the development of international cooperative archaeological enterprise, which, in turn, may determine what contacts are allowed to flourish and which are doomed to failure (Ucko n.d.).

Despite such variations in both practice and development, the mere fact that the archaeological discipline is seen to be a western 'export' often carries its own messages to those at the receiving end. Andah (Ch. 5) is particularly scathing about the inevitability of the inappropriate archaeological theory which

necessarily follows, and in its turn dominates, in the wake of the 'colonizers'. Likewise, Politis (Ch. 9:226–7) concludes:

> The histories about the past of South America…are coloured by a North American and European perspective and were designed, consciously or not, to satisfy the needs of western scholarships. Certainly, the agenda has not been set in South America. Research topics, objectives and methodologies have basically been produced in the United States and secondly in Europe. From there, they have been introduced into South America, and viewed as parameters for the scientific validation of local research. Standards regarding what is right or wrong, out-of-date or fashionable, methodologically correct or incorrect, are established outside South America.

However, nothing is perhaps as inevitable as Andah, at least, suggests. As Kuklick (1991:26) puts it:

> Even if we grant the premise that anthropology was born of the colonial situation, we are obliged to recognize that the permutations of the colonial situation admit of highly variable relationships between the representatives of cultures in contact, and that these relationships can foster self-doubt as well as arrogance. How else can we explain contemporary anthropologists' drive to redefine their craft?

Thus, for example, as Paddayya (Ch. 6) points out, it was the results of colonial archaeological discoveries which enabled the Bengali renaissance and subsequent Bengali Revolt. Nevertheless, Paddayya's generally positive perception of the colonially derived practice of archaeology in India can also be seen through quite different eyes:

> The period of 1860 to 1877 saw a rapid expansion of what might be thought of as the definition and expropriation of Indian civilization by the imperial rulers…. Through this period more and more Europeans came to define what they thought of as the uniqueness of Indian civilization…. In the 1860s an archaeological survey was established, with Europeans deciding what were the great monuments of India, which monuments were fit for preservation or for description as part of the Indian 'heritage'…. The British believed that Indian arts and crafts had entered a period of sharp decline in the face of western technology and machine-made products, hence their arts and crafts had to be collected, preserved and placed in museums…. The British rulers were increasingly defining what was Indian in an official and 'objective' sense. Indians had to look like Indians.
>
> (Cohn 1983:182–3)

As will be seen below, Andah's perhaps overgeneralized suspiciousness also fails to recognize that there are real attempts, within modern archaeological theory, to divest it of accompanying hangovers of Western domination of others (e.g., Shennan 1993).

In each of the examples that follow, it can be assumed that those archaeologists or agents of archaeology involved carried with them their own pre-conceived intellectual baggage and their own theoretical approaches to the evidence of the past. The nature and extent of their influence will have been tempered by the attitudes and perceptions of the past that existed—whether in the form of oral history or written records—in the countries to which they travelled. One may imagine a whole host of vested interests and cultural assumptions behind the bold historical observation that, for example, it was Charles III of Spain—who had previously, while king in Naples, known Herculaneum and Pompeii—who sent Captain Antonio del Río to Palenque (Mexico) in 1786 and who was almost certainly also behind the creation of an Indian museum in the University of Mexico (Lorenzo 1981:140–1). More recently, it was Foote's knowledge of the palaeolithic deposits of the Somme in France that apparently led Indian archaeology to search for and recognize stone tool characteristics and developments, and to adopt a more topographically based approach to archaeological exploration and analysis (Paddayya, Ch. 6). Another example was the introduction of the 'Vienna School' of archaeology into many countries (and its longevity in places such as Uruguay) long after it had lost all influence and credibility in Europe, which was the result of an extraordinary set of events, and individual personalities.

Historical examples such as these can also illustrate the complex events that have lain behind some of the individual influences on the archaeology of other countries. For example, Indonesian archaeology only became organized because of Brandes, and Brandes only became interested in Indonesia because the European art market chose to class Indonesian artefacts as 'works of art' (Tanudirjo, Ch. 3). In southern Africa it was the unusual combination of the commercial and other interests of Cecil Rhodes which dictated the development of archaeology in much of the region (Hall, Ch. 1).

Sometimes the individual archaeological agent appears transformed by the cultural contexts of his or her endeavours. For example, according to Paddayya (Ch. 6), James Fergusson was responsible for founding, and setting the scientific standards for all subsequent analyses of Indian architectural styles. From an Indian perspective, here is an outstanding scholar. Yet this is the same 'indigo merchant…[who] came to the study of Avebury…[with the] main purpose to throw contempt on the 'Orphite heresy'…[and whose] opprobrium reached almost paranoid levels…[and whose] own theory for Avebury appears no less dubious, based on a strained reading of the evidence, in ignorance of dating clues which now seem crucial' (Ucko, Hunter, Clark & David 1991:251). Fergusson's claims that not only Avebury and Silbury Hill were post-Roman in date, but also

the burial monument for King Arthur s dead soldiers, makes the Indian claim astonishing to English ears.

The work of Gordon Childe represents perhaps the most extreme example of the extent to which one individual has influenced modern archaeology. Retrospective classification and interpretation of supposed theoretical allegiances are sometimes difficult to take seriously and, perhaps, Trigger had his tongue in cheek when he wrote the following:

> Childe is not an ideal subject for even an intellectual biography. He destroyed most of his papers and correspondence when he retired…. There was also considerable disagreement during his lifetime about what Childe really believed…. Childe's interpretations often were based on very little archaeological data…. At the beginning and end of his career, Childe, as a culture-historical and *a prototypical post-processual* archaeologist…
>
> (Trigger 1994a:9, 10, 24—my italics)

Despite the fact that Childe was an impossibly bad lecturer (Kilbride-Jones 1994: 136–7), a bastion of non-cooperation between archaeology and anthropology (see Hodder 1991b:11), and the epitome of inward-looking Eurocentricism about the past (Mulvaney 1994; Gathercole 1994; Trigger 1994a:12–13), his work is *still currently* by far the most profound individual influence on international archaeology, from Ireland (Cooney, Ch. 12) to Japan (Tsude, Ch. 14). In some cases the explanation for the continuing influence of Childe's works—and often, just one or two of Childe's works—is that they have only become known to specific countries in the relatively recent past (e.g., Argentina and Portugal, after the mid–1950s—Politis and Jorge & Jorge, Chs 9 and 11; Indonesia in the mid–1970s—Tanudirjo, Ch. 3). In general, however, Childe's continued international popularity must reflect the ongoing world acceptance of archaeology as part of historical enquiry and the desire to establish culture histories. Childe's approach was exclusively one based on an evaluative comparative method (e.g., Harris 1994). In addition, as Trigger (1994a:24) says, and this perhaps has added to his continuing influence, Childe constructed 'a vision of archaeology that was as broad as that of the other social sciences but which also took account of the particular strengths and limitations of archaeological data'.

Individual contacts (whether through publication or collaborative ventures) lead to new enterprises and the development of theory and it is in this context that recent debates about the usefulness of international conferences (whether large or small) have gained an interesting gloss, for there can be no doubt that such interactions have also led to important collaborations and the mingling of archaeological traditions of investigation and interpretation. For example, it has been through the medium of conferences in which those such as Binford and Hodder have participated, that India (Paddayya, Ch. 6) and Portugal (Jorge & Jorge, Ch. 11), and probably other parts of the world and other archaeologically peripheral areas of Europe, have acquired their knowledge of, and interest in,

aspects of new developments in archaeological theorizing. It is also in this context that the active participation of younger academics in archaeological decision-making, and their exposure to a 'world' experience (Ucko 1987; and see below), becomes so important. Attempts to achieve international collaborations—let alone the involvement of the young in such collaborations—can, however, be frustrated by the organizational and traditional bureaucracies of different national archaeologies (e.g., the separation of academies from universities (see Dolukhanov, Ch. 16; see Härke, Ch. 2)).

Given the range and character of various influences, it is clear that changes in archaeological attitudes, theory and application are likely to occur in fits and starts (and see Kuhn 1970). This is demonstrated by Moser's (Ch. 7) detailed case study of a twenty-year period in the development of the Australian Institute of Aboriginal Studies, which reveals a wide variety of influences and causes of change such as politics; the beliefs and actions of specific individuals; legislation; and other individual and culture-specific perceptions of events.

REGIONALISMS

The importance of 'chance events', and the singular roles played by certain individuals in carrying a particular theory and practice of archaeology elsewhere, already suggest that it would be rare to find many straightforward correlations between regional histories of cultural influence, and the nature of the sort of archaeology that had been adopted in the region concerned. Although there seems no doubt about the direct Dutch influence on the Indonesian archaeological enterprise (Tanudirjo, Ch. 3), nor about that of Germany on Namibian archaeology (Kinahan, Ch. 4), such cultural imperialism has been neither necessarily continuous, nor straightforward. Thus, in Indonesia, Dutch influence has been recently displaced by training programmes, publication subsidies and actual personnel from the United States (Tanudirjo, Ch. 3). In Latin America, individual countries have been subjected at various times to a variety of different foreign archaeological influences (e.g., Chile: the United Kingdom, Belgium and the United States), perhaps the most powerful shared experience being the most recent, that from the United States (Politis, Ch. 9). Sweden's anti-colonial activities in eastern Africa (Sinclair 1989), or Ireland's inheritance of Austrian and US archaeological practice and theory (Cooney and Woodman, Chs 12 and 13) are all events which have been at least as important for the development of theoretical archaeology in the areas concerned as the British colonial experience.

The actual complexities of events leading to particular regional archaeologies are interesting in their own right. P.Funari (pers. comm.) tells me that, despite the common asset of the Portuguese language, archaeological scholarly links between Portugal and Brazil have been almost non-existent. Up to the 1980s, the only links between them were the scientific journal *O arqueologo portugues* received by some Brazilian institutions (although very seldom quoted there), and

publications on heritage and the Roman urban site of Conimbriga (see Jorge & Jorge, Ch. 11). Brazilian archaeologists have preferred to be in contact with mainstream European and North American trends rather than with Portuguese practices. Historically, Portugal has been as peripheral as Brazil; if anything, there has been a somewhat closer relationship between Brazilian archaeology and Latin American archaeology than between Brazilian and Portuguese archaeologists. Strangely enough, it is probable that Brazilians would only read Portuguese writings when, and if, the authors have previously published in Britain or France. More significant has been the recent introduction into Brazil of Portuguese translations of introductory books by foreign authors such as Møberg (Sweden), Leroi-Gourhan (France) and Childe (Australia/UK).

Politis (Ch. 9:223) demonstrates an equally complex, and strained, relationship between Latin American and Spanish archaeologists:

> When archaeology became a scientific discipline in the continent, South America was no longer under Spanish political and economic control. Some aspects of 20th century Spanish intellectual life, such as literature and philosophy, certainly influenced South American societies but the impact of this was generally confined to the arts and humanities, and did not make itself felt within the social sciences.

Even since the close contacts of the 1960s between Argentina and Spain, there has been no exchange of archaeological theory (Vázquez Varela & Risch 1991) between the archaeologists of the two countries.

In other words, no simple equation such as that outlined in the original EuroTAG programme (see Preface) is sustainable. Indeed, other sociopolitical factors are likely to be just as significant as a shared language in determining attitudes towards one 'type' of archaeological interpretation rather than another. Thus, although Brazilians and Latin Americans are usually considered as separate conceptual entities, Portuguese—and Spanish-speaking countries in the Americas share a common identity challenge: how to cope with the fate of mimicking the elites' desire to be considered European through the material evidence of the past (Funari 1994a), while 'the representation of the Indian as either noble or villain requires that he remains mute about himself, a passive figure to be moulded by European ideologies, conflicting as these may be' (Ramos 1994:84).

Up to now, the importance of differences in regional archaeological developments has been seriously overlooked:

> What has not yet been studied adequately is the nature of the divergences [of regional or national traditions in archaeological interpretation]. To what degree do they represent irreconcilable differences in the understanding of human behaviour, differences in the questions being asked, or the same basic ideas being studied under the guise of different

> terminologies?… On close inspection, most interpretations by archaeologists working within different national traditions can be assigned to a limited number of general orientations…. I have identified three types: colonialist, nationalist and imperialist or world-oriented.
>
> (Trigger 1989:8–9)

Contrary to Trigger's (1989:9) claim that 'It seems unwise to over-estimate the independence or theoretical distinctiveness of these regional archaeologies', and as also recognized by Sherratt (1993:120), regional archaeological epistemologies do indeed need to be further analysed, differentiated and, certainly, understood. Thus, for example, Härke (Ch. 2) stresses the absence of any tradition in Germany for examining the history or context of archaeological enquiry—a deficiency to be hidden away, though in no way convincingly, within the 'regional' (?German) (perhaps convenient) claim that objective analysis of a discipline cannot be attempted by practitioners of the discipline concerned (and see Härke & Wolfram 1993).

If Europe is indeed a region (and see Graves-Brown, Jones & Gamble 1995) then its various regional traditions of theoretical archaeology are strikingly distinctive. As Härke (Ch. 2), Jorge & Jorge (Ch. 11), Cooney (Ch. 12), Woodman (Ch. 13), Thomas (Ch. 17) and Olivier & Coudart (Ch. 18; and see Hodder 1991b:20) demonstrate, archaeological developments in the different countries of this 'region' are so distinct from one another as to be almost on a different planet of assumption and activity. Wherever one looks, for example in South America (Politis, Ch. 9; Funari, Ch. 10), one discovers archaeologies driven *variously,* and to *different* extents, by ethnicity, by heritage concerns and by nationalism.

NATIONALISMS AND CULTURE-HISTORY: INDIGENISM, ETHNICITY, POLITICS AND CHANGE

As analysed by Evans (Ch. 15), much of immediate post-World-War-II discussion was (not surprisingly) concerned with questioning the role of the state in 'controlling' 'national' archaeologies and, as he reports, there was little consensus then. Sherratt (1993:121) links state control of archaeology with wider international movements of such control. However, in the 1990s state control of archaeology in Portugal (Jorge & Jorge, Ch. 11) is seen to be a potential source of increased financial support, greater administrative efficiency, and more regional devolution; many other countries have long since accepted the inevitability of state control. In the united Germany of today (Härke & Wolfram 1993; Härke, Ch. 2) this still leaves in place a strictly hierarchical control of archaeological activity and archaeological interpretation—coupled with a civil service mentality and approach to the practice of the discipline. The evidence is that the vesting of archaeological enquiry in the 1990s within a national state agency leads to two consequences: first, to a real fear for the future of archaeology as public monies

dry up and other priorities receive greater state support; second, to the fostering (whether it be in Germany (Härke, Ch. 2), India (Paddayya, Ch. 6), Japan (Tsude, Ch. 14), the former Soviet Union (Dolukhanov, Ch. 16), or the United Kingdom) of an approach to archaeological fieldwork which assumes (in a good old-fashioned Pitt-Rivers-type way, and often using Pitt-Rivers methodology) that archaeological facts are out there to be recorded objectively according to a series of always improving strategies and technical skills.

When it comes to existing theoretical literature on the role of archaeology and the nature of different nationalisms (e.g., Rowlands 1994), little has changed on the global scene since Trigger announced his somewhat unsatisfactory and superficial three-fold classification (quoted above). It is the variation, rather than the common ascription of over-arching western classificatory categories to archaeological diversity, which may be significant (see, e.g., details for Indonesia, Namibia and Ireland—Tanudirjo, Kinahan and Cooney, Chs: 3, 4, and 12).

At another level, also, comments on nationalistic archaeology, and the theory which drives such archaeology, are still all too often presented at an unacceptable level of generalization. In the hands of some (particularly British?) archaeologists, all archaeologists within such regimes are patronized and stereotyped:

> Archaeology was until very recently still conducted by the Institute of the History of Material Culture maintained by the Soviet Union and its associates in the pious hope that, pursued with sufficient zeal, it would ultimately validate the historical philosophy developed by Karl Marx during the middle decades of the nineteenth century.
>
> (Clark 1992:101)

In reality, of course, where archaeological theory is considered a worthwhile pursuit at all (see below), it will be discussed with intelligence and sophistication by those who attempt to practise it, whether within the constraints of Nazi Germany or the former Soviet Union (Härke & Wolfram 1993; Härke, Ch. 2; Evans, Ch. 15; Taylor 1993; Dolukhanov, Ch. 16), or as part of the American ‘colonial’ export to South America, via Meggers and Evans, of a ‘naive’ ecological positivism (Politis, Ch. 9; Funari, Ch. 10) or in the context of nationalistic choices about ethnicity in Ireland (Cooney, Ch. 12).

The apparently convenient use of the term ‘nationalism’—which, in itself, is in any case not a unitary phenomenon—must not be allowed to hide the dynamic change which can occur within such ‘nationalisms’. As Woodman (Ch. 13) points out, Irish archaeology has been, and undoubtedly will also be in the future, influenced by the nature of the existing relationships between Belfast, Dublin and London at any particular moment: so, to suit the circumstances, ‘acceptable’ (or otherwise) Picts and Celts have emerged, and so too have ‘inappropriate’ monuments been removed from sight. In Germany (Härke,

Ch. 2), archaeology has at different times been variously asked to 'validate' a German background of Greeks, Romans or Aryans (and see Trigger 1994b:51). In Brazil or Zimbabwe, it has varied from Phoenicians to almost anyone! Nationalisms are not necessarily static affairs; as Bond & Gilliam (1994:4) have explained in the context of the making of their own histories by peoples in countries such as those in eastern Europe, Africa and Latin America:

> Interpretations of the past are an important feature of the political struggles for their individual and collective identities and their claims to power and economic resources. The struggle extends to the use and meanings of dominant icons, images, discourses and written texts. It centres on the manner in which we understand and represent relations of inequality.

Existing theory about the way that nationalisms create the archaeologies which may be of use to them has not yet come to terms with the way that 'indigenism' may develop political influence of a kind and degree which itself demands a new defining of 'national/state interest', involving revised, or at least differentially evaluated, references to the evidence of the past. A striking example comes from Australia where, first, the granting of the vote to Aborigines, and subsequently, the conversion from a national policy of 'assimilation' to one of Aboriginal 'self-determination' (including, in particular, legislation for Land Rights) has profoundly altered—both in the short *and* the long term—the focus, attitudes and practices of the archaeological discipline (Ucko 1983), leading, amongst other things, to the redefinition of what should constitute the meaningful definition of a 'significant site' (Ritchie 1994; Ucko 1994a; Moser, Ch. 7). In the United States, a revolution in archaeological research ethics and archaeological curating—based on a national recognition, or even redefinition, of the rights of Native Americans—is well underway (Morell 1994; and see Mackie, Ch. 8, for Canada).

As will be seen below, the politics of indigenism—so often focused on the claim for primacy of land ownership—frequently runs directly counter to nationally accepted versions of what is deemed to be significant from the past, judgements which are usually based on a culture-historical interpretation of archaeological evidence. Dogged by the spin-offs of diffusionary and migrationary assumptions of such culture-historically assumed, value-laden social and developmental hierarchical models, those very groups who have been dominated for so long in so-called 'Fourth-World' contexts often have to choose between negating the values of archaeologically-derived culture history and renegotiating the values ascribed to particular stages of such culture-historical development in order to possibly regain some part of 'their' excluded pasts (Stone & MacKenzie 1990; Stone & Molyneaux 1994).

Politis and Funari (Chs 9, 10) make it clear that such political considerations and choices are not limited to the indigenous minorities of the Fourth World, but can apply also to (socio-economically disadvantaged) ethnic groups anywhere in

the world. Thus, for example, in Brazil, ministers, secretaries, museum directors and other bureaucratic intellectuals continue to claim that the country is made up of 20 million citizens and 130 million inhabitants (i.e., non-citizens—most of the Blacks, and all of the Indians). The Brazilian Paulista Museum, and its archaeological displays, continues to be an excluding and elite-oriented institution, exactly as it was a century ago (Funari 1994b:124–9). In all these situations there are ongoing, and essential, political debates to be resolved. Archaeological evidence—because of its tangible point of reference—will no doubt continue to be used as the basis for claims of ethnicity and nationalism (Rowlands 1994:141).

It is widely recognized, at least in the Anglo-American-Australian-Canadian world, that the bastion of nationalisms has been the culture-historical approach to the past (i.e., the collection of data as empirical evidence of what took place in the past). Within this broad church there has been some variety, usually focused on the relative strengths of evolution versus diffusion/migration as the explanatory mechanisms for the 'evidence' 'revealed' by the 'facts'.

Such culture-historical interpretation is often excluded by some Anglo-American theorists from the category of 'theory' or ideology: 'It can more generally be claimed that culture history is a methodology rather than a theory although of course it contains theoretical assumptions' (Hodder 1991b:4; but see Tschauner 1994). Yet, convenient though such exclusion might appear to be, it is difficult to sustain the distinction from other 'theory', since: 'of course all theory is to some degree socially embedded and pragmatic…' (Hodder 1991b:7, and see Hodder 1991b:4). Thomas (Ch. 17) effectively summarizes the basis of culture-historical approaches:

> They are largely concerned with the extraction, description, classification and compilation of archaeological evidence relating to a particular period, or amenable to a particular kind of scientific analysis.

Many nationalistic archaeologies have turned their interests firmly away from all *non*-culture-historical archaeological 'theory'. They appear to have done so for a variety of reasons and in different social conditions and probably not simply because, as suggested by Hodder (1991a:x), the adoption of an atheoretical refuge was a reaction to their previous uncritical adoption of German 'theory'—(shades of Freud as an explanatory model?) (Härke, Ch. 2). In Ireland, for example, the feeling is that its culture-historical archaeology is very successful in accomplishing its perceived role of supporting Irish identity, 'constantly coming up with new information' (Cooney, Ch. 12; Woodman, Ch. 13). As in Ireland, so also, for example, in India and Japan, archaeology is seen to be an empirical activity, a culture-historical fact-gathering exercise (Paddayya, Ch. 6 and Tsude, Ch. 14), with 'theory' (in itself) cynically considered to be merely a Western distraction. In Germany, 'rejection of ideology has itself become an ideology' (Härke & Wolfram 1993; Härke, Ch. 2). As Politis (Ch. 9) explains for Latin

America, against a back drop of national coup and counter-coup, 'the production of [any] theory is usually seen as a 'foreign country', while daily [culture-historical] practice is a means to survive and, at least, to keep some dreams alive'.

The culture-historical basis of archaeological interpretation has often been based on equations of material culture complexes with assumed language and assumed biology—the supposed 'peoples' of the past (and present) (see Härke, Ch. 2; Kinahan, Ch. 4; Dolukhanov, Ch. 16). Many claim that we have been long aware of this, and of the frequent linkages of racism, material culture, ethnicity and nationalism. Yet, even today, few have realized the extent of the equation; thus, it is still shocking to read Hall's (Ch. 1) evidence for the way that racist assumptions underlay the 'objective' culture-historical interpretations by Caton-Thompson and others of Great Zimbabwe, or to be forced to remember the nature of Wheeler's pro-Indian remarks (Paddayya, Ch. 6) which could, so easily, be misinterpreted in Africa:

> In India it is possible to dig almost anywhere below a living level and to discover the vestiges of civilization layer by layer. That is not of course true of a great many regions of the world. Large expanses of Africa, for example, would be singularly unresponsive to a crude test of this kind.
>
> (Wheeler 1976:66)

Such culture-historical/nationalistic/racist approaches to archaeological evidence have led to archaeological practice being carried out in 'peculiar' ways: in Ireland, emphasis has been on the search for anything distinctive in the archaeological record that would prove that the nation was never at one with England (Woodman, Ch. 13); similarly, in Portugal, the focus has been on anything that would demonstrate cultural separation from Spain (Jorge & Jorge, Ch. 11). Whether dangerous or not in itself, it is worrying that there is so much evidence to show that the culture-historical frame-work for representing the past remains the same even when those in power change (even from colonial to post-colonial new nation states) (Ucko 1994c; and see Graves-Brown, Jones & Gamble 1995). The use of culture-historical archaeology to provide evidence of a continued elitism of certain peoples (e.g., attitudes *vis à vis* 'Bushmen' in the museum displays of southern Africa), and as revealing disjunctiveness from others, appears to be a continuing trend today (Hall 1994; Hall, Ch. 1; Kinahan, Ch. 4; Mazel & Ritchie 1994; Ucko 1994b; Ucko 1994c).

At its most insidious (when coupled with theories of migration), such archaeology, as seen above, has been able to deprive whole peoples of any legitimate past. Amazingly, this kind of culture-historical interpretation is still strong in Ireland (Cooney, Ch. 12) and many other parts of the world where, for example, it continues to stereotype the indigines of Indonesia and elsewhere as static and uninventive (Tanudirjo, Ch. 3). Even in India, which has begun to adopt some elements of post-positivist archaeological theory, it is the culture-

historical perspective which is considered the most suitable, at least for initial enquiry into new unplotted areas or regions of the sub-continent (Paddayya, Ch. 6). As Härke (Ch. 2) rightly points out (with regard to the domination of European archaeology by this particular German contribution to the subject), much more research is needed on the reasons for the longevity and persistence of the attraction of this culture-historical parameter of archaeological interpretation (and see Jones 1994). As Trigger (1989:205) concludes:

> European archaeology became closely aligned with history and was seen as offering insights into the development of particular peoples in prehistoric times. Its findings thus became part of struggles for national self-determination, the assertion and defence of national identity, and promoting national unity in opposition to class conflict. Archaeology of this sort obviously had a wide-spread appeal in other parts of the world… only an approach that is focused on understanding the prehistory of specific peoples can fulfil the needs of nations in a post-colonial phase. For this reason culture-historical archaeology remains socially attractive in many parts of the world.

BEYOND CULTURE-HISTORY, THROUGH THE 'NEW ARCHAEOLOGY', TO POST-PROCESSUALISM

For those involved with the 'New Archaeology' in the 1960s and 1970s, those were indeed 'heady' 'theory' days—now relegated in the current theory debate almost to oblivion. It seems obligatory that claims for 'new' theory demand the downgrading of past thought and endeavour (in whatever discipline; and see Wylie 1993:21). Perhaps the Germans had a point when they claimed (Härke, Ch. 2), when rejecting the 'New Archaeology', that its authors were not only abrasive (e.g., see Kohl 1993), ill-read and exclusively anglophone, but also that the spread of the 'New Archaeology' appeared to spawn a "multiplicity of short-lived fashions' (see Chippindale (1993:33) for the claim that so many archaeological theories of whatever ilk are 'briefly modish, and are then ditched for their failings').

Perceptions change regarding what anyone—or any group—is, or should be, doing within theoretical archaeology, both in practice and in theory. Hodder's (1991b:11) recognition of the selectivity of adoption of ('New Archaeology') processual ideas by different countries can be exemplified by Ireland's choice of only certain items from processual, and certain others from post-processual, archaeological theorizing—conveniently all lumped together by the Irish as 'New Archaeology'—to add to their own very empirical archaeological interpretative tradition (Cooney, Ch. 12). This contrasts with India's 'New Archaeology' call for cultures to be conceived of as adaptive systems, for the carrying out of intensive field surveys, and for the use of ethnoarchaeological models for the reconstruction of settlement systems (Paddayya, Ch. 6). In Japan (Tsude,

Ch. 14: 307), particular bits of processual archaeology have been adopted and modified to become part of a Japanese 'scientific archaeology':

> Contextual archaeology seems to have emerged as a sort of digestant for Anglo-American archaeologists who have suffered from stomach-ache after eating too many heavy steaks called processual archaeology. Such medicine may be felt to be unnecessary for most Japanese archaeologists, who have tried a small tasty portion of the steak.

It seems much more a question of choosing what seems suitable to particular contexts than, as Kohl (1993:13) appears to suggest, everyone having to go through the same 'theoretical developmental stages' as Anglo-American archaeologists to reach a different set of perceptions about the appropriate way to approach the past (and see Hodder (1991b:16) and Ucko (1992:xi) for a similar question with regard to computer applications to archaeology).

Of course, the adoption of any particular theoretical approach involves the selection of what is considered to be the most appropriate from a plethora of available academic or social ideas. Perceptions are bound to vary about the nature of what is, or is not, appropriate. For example, many post-processual archaeologists (e.g., Hodder 1991b:12) have by now recognized that US-mid-west-derived processual interpretations, with their emphases on environmental or ecological explanation (e.g., homeostatic adaptive systems), may well have contributed to 'imperialist' interpretations of the pasts of Africa (Andah, Ch. 5), Canada (Mackie, Ch. 8) or Indonesia (Tanudirjo, Ch. 3)—pasts they (therefore) interpreted as having been the material culture evidence of peoples with little innovative capacity. As such, in the eyes of many Africans or Canadian First Nations, or Indonesians, this 'processual' new Anglo-American theoretical fad could be seen to be no more appropriate to their pasts than was the earlier imposed straitjacket of the European-derived 'Three Age' System or the imposed European-derived 'Vienna (migratory) School'. Many post-processualists would no doubt be amongst the first to see all previous theory as poor attempts to impose a fictitious order on a mass of data.

One may suspect, however, that all—or most—theory can be interpreted, by those to whom it is applied, as imposed, constricting or inappropriate. Thus, for example, post-processual 'structuralism' and/or 'cognitive archaeology', as employed in the interpretation of Nigerian or Zimbabwean archaeological prehistory, can be accused of being an essentially static European construct again being applied to others in order to denigrate their own indigenous potential for change (a potential which they assume to have existed as much as any past European—individual or culture). That is, they are just other ways of assigning to these areas of the world an *ahistorical* frame-work of explanation, unthinkingly (uncaringly?) incorporating creations based on previous European theory, such as the stereotype of the static Bantu (Hall, Ch. 1; Kinahan, Ch. 4; Andah Ch. 5). Much the same attack has been directed against what used to be

the ubiquitous 'colonial' narrative device of the equally static ahistorical 'ethnographic *present*'.

Whereas, occasionally, the 'theory of the day' may become the tool of resistance (Julian Thomas, pers. comm.), it is clearly essential that the choice of the most 'suitable' theoretical approach not be left exclusively in the hands of the dominant elite of any society. As Tsude (Ch. 14) points out, the archaeological request to investigate Japanese royal mausolea has met with no success and, indeed, it is only since the 1950s that Japanese orthodoxy regarding the racial homogeneous distinctiveness of Japanese culture has allowed the evidence for wet rice cultivation to be considered in the context of a posited influx of people/ideas from outside Japan, rather than as an indigenous Japanese development. This is in sharp contrast to Namibia (Kinahan, Ch. 4) where South African/German orthodoxy sees all archaeologically attested cultural *innovation* within Namibia in terms of the 'influence' (at least) of outsiders.

It is even possible to imagine that the elaborate theoretical terminology adopted in some processual and post-processual writings could be accused of being a last bastion of a new defence by the Anglo-American archaeologist to keep every one else at bay (and see Hodder 1989).

ACTION OR EXTINCTION

In England, Wales and Northern Ireland, in a rare example of academic archaeological consensus, agreed syllabuses (including 'Theory') now exist for the core subjects of the archaeological discipline at both school and university levels (Austin 1987; and see Stone 1992). It is the aim of all these programmes of formal education (for whatever age) to develop critical faculties to enable self-assessment of the qualitative and contextual nature of the data about the past. This is much more hopeful for the well-being of the long-term future of the archaeological discipline than was the case previously in the United Kingdom, when the past was exclusively a matter of learning alleged 'facts', alleged dates and places of 'first discoveries', and geography and dates of alleged migrations of peoples.

In the past (see Evans, Ch. 15), the theoretical context of archaeological interpretation allowed for the dream of an archaeology in education which would reveal—through the 'evidence' of archaeological enquiry—a unified humanity. Today, rather, the archaeological message might be the open, and self-critical, recognition that the empirical evidence of the past is an account created—on the basis of whatever theory (part, or group, of theories) may be in vogue at any particular moment in any particular corner of the world—and therefore that the archaeological evidence of the past cannot be an appropriate basis for claims (at least of any 'traditional' kind) of racial or national superiority over others. Indeed, perhaps the most potentially exciting present educational developments are those foreshadowed by the move to 'community-based' archaeologies (see Sinclair 1990; Nackerdien 1994; Ucko 1994b; Ucko 1994c; Moser, Ch. 7) based

on 'relevant' theory (i.e., theory seen to be applicable to the local peoples most concerned with the evidence of the past)—despite all the difficulties archaeologists have in coming to terms with concepts of 'relevance' when the majority of the world's archaeology is still based on naive assumptions about the 'objectivity' of its empirical discoveries.

There are real potential problems with the present state of archaeological theorizing, problems which can only be sorted out by archaeologists themselves. The problem can be well expressed through two quotations from the same book:

> Most often, scholars attempt to remove themselves from the fray of public combat and should their interpretations or assessments prove false or inappropriate, they usually stand to lose very little indeed. The formulations of scholars are rarely tested within situations of actual social turmoil. Rarely do they have to pay for their misinterpretations.
>
> (Bond & Gilliam 1994:11)

> So an archaeologist who writes within a national or ethnic framework cannot help but take a critical stand as to how his/her work is used. There seems little doubt that this is a growing dilemma for archaeologists.
>
> (Rowlands 1994:134)

This is a far cry from the situation prior to the 1980s, at least in the West, when the assumed empirical 'objective' nature of archaeological evidence could allow the archaeologists merely to stand aside or to claim that the 'truth' was being manipulated by 'others' for their own political ends. Nowadays, on the contrary, many archaeologists reveal their various enthusiasms for the potential active role of archaeology and archaeologists in their own different contexts and parts of the world: for Canada, Mackie (Ch. 8) demands a morality and relevance if the future of archaeological enquiry is to be secure; for Brazil, Funari (Ch. 10) sees archaeological theory as playing a vital role in challenging accepted orthodoxy; for Portugal, Jorge & Jorge (Ch. 11) see a future eclecticism, and the possibility of real choice between competing parameters of interpretation; for western Africa, Andah (Ch. 5) dreams of an archaeology which adequately captures the distinctiveness of *African* tradition and practice. The hope, therefore, for the archaeology of the 1990s, and after, is that many archaeologists in many parts of the world have now really recognized the socio-political dimensions of the practice of archaeology. Clearly this is an immense step away from the days of 1985 when the West-European-/North-American-dominated International Union of Pre- and Proto-historic Sciences (IUPPS) was still powerful enough to maintain its fiction of a factual, objective archaeological science. The trouble is that recognition of the socio-political influence on the archaeological discipline does not lead to inevitable success in removing its influence from our interpretations of the past. Nor, more discerning though we now are, is it self-evident how we should seek to convince archaeologists in South America, the

former Soviet Union or Africa that current offers of assistance from the United States to help detect and record archaeological sites within tropical forests may reflect wider political aims than disinterested fraternal, collegiate activity. Indeed, to so convince would leave our ignorance of tropical forest areas intact—thus, in turn, perhaps depriving whole groups of peoples of a putative past 'heritage', as well as depriving local archaeologists in such areas of an exceptional chance to form part of the closed western shop of 'objective *scientific* archaeology'! (And who would be brave enough at this time to go further than Thomas (Ch. 17) when he says that 'one might be less than enthusiastic about seeing Anglo-American thought transported intact in the Third World' (and see Hodder 1991b: 16), and actually try to justify to the Cameroon, or wherever, why a Swedish aid agency, or a British university collaboration, should be considered more reliable (=less political?) than an American agricultural or forest service?).

To make things even more complicated, the 1990s have yet another actor on stage; from country to country in every continent, the 'heritage', and associated tourism, is either a source of hope for change (Portugal—Jorge & Jorge, Ch. 11) or a fear for the future priorities of archaeological enquiry (Ireland—Cooney, Ch. 12). For Namibia (Kinahan, Ch. 4), as also within Zimbabwe (Ucko 1994b), the heritage presentation which links live dancing performers to static rock art display in a (German) museum is a depressing continuation of an old theme, the creation of a fictitious static past (and see discussions leading up to, and including, Mazel 1993) for those whom archaeologists chose to study. More than this, as Bond & Gilliam point out with respect to several chapters in their book (Bond & Gilliam 1994:4), 'supporting "traditional" precolonial customs and practices may well appear as resistance but in fact they are acts of collusion and subjugation'. We are only now beginning to work out the potential profound implications of a really effective heritage-led/driven archaeology, accompanied by its powerful baggage of mass media presentation, for whole areas of interpretation (Sherratt 1993:124; Hall, Ch. 1; Paddayya, Ch. 6), as well as for archaeological funding (Champion 1991). It seems abundantly clear that Irish archaeologists (Cooney, Ch. 12) will not long be allowed to remain aloof from the spheres of public education and public site presentation, claiming that their role is to be solely as guardians of accuracy about the 'facts' of the past; indeed, Woodman (Ch. 13) insists that Irish archaeologists will have to become stronger in order to be heard at all.

THE FUTURE

Rowlands (1994:132–3) strikes a note of realism about the future of archaeological interpretation, by accepting that the subject nowadays has, like it or not, a major new emphasis and context:

> The manipulation of archaeology in the shoring up of identities is now far more widespread than in the 1930s when Kossinna-like racial arguments

> stalked the archaeological landscape. Whether in the form of cultural heritage, where the production of archaeological identities might be seen as admirable in empowering local groups and indigenous rights or in cases of ethnic nationalism where archaeological accounts of the past may be distorted to serve political goals that most would find distasteful and objectionable, identities are produced as categorical imperatives to serve political ends. This is far removed from the naive, unreflexive 'good old days' of empiricist archaeology but is consistent with the general relation of intellectual work to society in the 1990s.

The question is whether the archaeological enthusiasms, expectations and fears of the 1990s, as we have seen them across the world, are of a different kind from hitherto, likely to be able to encompass developments such as 'renewed' ethnicities (Jones 1994), 'new' states, 'cultural tourism' and heritage legislations. Essentially, will the discipline of archaeology be able to formulate its own disciplinary criteria which will indeed allow it to distinguish between the 'alternative' and the 'distorted'? Will the archaeological discipline of the mid–1990s be successful in creating a set of internal standards whereby its assessments concerning the past are accepted as being qualitatively more than the mere support of, or opposition to, the political interests of the day? It is in this context that the potential importance of the world archaeological movement (through the World Archaeological Congress (WAC)) may prove to be of the utmost significance; Evans' (Ch. 15) apparent fear of the potential divisiveness of the myriad potential ethnic voices of the archaeological future fails to take into account the *ongoing* nature of WAC activities (through its critical involvement of the international archaeological junior, and therefore the young, in its affairs, its publications and its global meetings) which discuss exactly those issues of ethnicity, identity and heritage which need to be moved into the centre of the public domain if archaeology is to be able to cope with the challenges of the future. Another significant development of the last few years is the precedent established by the Swedish Agency for Research Cooperation with Developing Countries in effectively vesting control of archaeological budgets in the hands of the Third World archaeologists who have the responsibility for the exploration of their own, and their country's or region's, pasts.

Archaeologists must accept that they now have a difficult new duty to perform. Those such as Ian Hodder (pers. comm., during a walk in Whipsnade zoo, many years ago) who see the vesting of public emotion in the past as more important than the particular version of the past which is to be presented as 'truth', will have to make public their acceptance that archaeological interpretation implies the possibilities of rivalries and fissions over who controls the past. In many cases, therefore, disunity between those who claim to read the evidence of the past should be interpreted more as a sign of the future viability of archaeology (Murray 1993:112; and see Ucko 1994b for Zimbabwe) than as a sign of its impending demise. Clearly, all this is uncomfortable, and a far cry from the

ongoing US strategy of attempting to control the archaeological pasts of others through offers of financial and 'high tech.' assistance (and see Politis, Ch. 9; Funari, Ch. 10), from ongoing IUPPS emphasis on chronological and temporal European sequences within a pretended world context, or the belief in the United Kingdom that ongoing support for British schools of archaeology is an appropriate continuing way of investigating the pasts of non-UK territories.

Strangely enough, it is exactly at this time that the importance of 'theory' cannot be simply dismissed as an irrelevant European or Anglo-American preoccupation; as we have seen, theory in the 'wrong' hands can divest a whole continent of its background, and/or of its potential identity reference point, and of its potential (new, or repackaged) nation—or state-hood.

Hodder (1991a:ix–x) makes the important point that:

> One of the most important aspects of an emphasis on archaeological theory is that it focuses our attention on concepts and taken-for-granteds used in the construction of the past—including past ethnicities. There is a need for a continual critique of reconstructions of the past as ideological. Nationalist and ethnic uses of archaeology to justify conflict need to be counteracted, as far as they can, with a wary and critical eye.

This is all the more important as we grow to recognize not only that new theory does not necessarily lead to a corresponding abandonment of racist assumptions (Hall, Ch. 1; Kinahan, Ch. 4), but that even when the archaeological evidence may suggest a likely picture of the past this may not adequately represent the relevant ethical position which the observer might wish to see adopted in the present (see Gilliam & Bond 1994, and Rowlands, quoted above). One of the most immediate, and apparently intransigent, archaeological problems in the 1990s is the case of the multi-period, Hindu and Muslim, religious site of Ayodhya, which Paddayya (Ch. 6), represents as 'total chaos…[at least, a] distorted use of the past'. In this instance it would seem impossible for the message from archaeology really to be about the universality of human culture, accompanied by the claim that the notion of autonomous histories was now obsolete. Rather, it would surely have to be about the uses and abuses made of the past in the short-term interests of the dominant politics of the day:

> There is a very fine line between historical/archaeological 'fact' and myth, with the distinction being made essentially in relation to contemporary issues and in the contemporary context. The past is thus continually recreated…[Ayodhya shows that this boundary], already flimsy, has collapsed and quite evidently the wrath of the people cannot be cooled by presenting them with so-called historical verities. Both groups [Hindu and Muslim] are today recreating separate pasts, those that are more in consonance with their contemporary world—a world in which, being

> insecure, they feel the need to crush any overt expressions of the identity of the 'other'.
>
> (Rao 1994:154, 161)

As if this 'new-look' message to the public about the past was not difficult enough, Shack (1994:116) has now made a complex argument even more complex; yet it is difficult not to agree with his sophisticated gloss: 'Fact becomes fancy. That too is irrelevant if in the course of time the hegemonic struggle for dominance is affirmed; the process of reconstructing the past on the assumption of truth asserts itself'.

But, for at least two good reasons, all is surely not lost. First, all societies have their own vested interests in the past, through such culturally defined focuses of interest as kinship, sites and oral history, quite apart from formulated rites and legislative procedures (and see Carmichael, Hubert, Reeves & Schanche 1994). And, although we know much too little about the details of what changes occur as a result of the arrival of archaeology, with its 'scientific' methods and linear dating methods (and see Murray 1993:112), one of the strong possibilities is that archaeological 'results' have established a set of static reference points where, previously, there had been an area for debate, compromise, negotiation and social discourse. Yoffee & Sherratt (1993) attack post-processual archaeology for its alleged inability to distinguish between competing alternative interpretations of the data, yet it surely should be an advantage to have a plethora of available theoretical approaches to be applied to the 'empirical evidence' of the past, from which the most suitable may be adopted, and/or adapted, by those with the most need to incorporate such an approach, or approaches (but see Rowlands (1994:140–1) for a series of cogent warnings about the potential abuses of such a situation—incompatible conclusions, ethically unacceptable adoption of a particular set of possible interpretations, etc.—warnings which serve to stress the need for archaeologists to adopt a series of internal disciplinary benchmarks).

The second reason for hope is that the 'comparative method'—that approach to archaeology and anthropology which has inspired so many of both disciplines at least from the nineteenth century to the 1950s (and see Evans, Ch. 15)—may not, after all, despite all dire predictions to the contrary, be dead (at least not in modified forms). Traditionally, today, 'practitioners' of the 'classic' comparative method are threatened by those aspects of post-processualism which focus *exclusive* interest on local-tradition-loaded significance, as they would also be by those such as Andah (Ch. 5) who insist on the unrivalled importance of *local* oral history and *local* 'cultural' tradition. However, Gamble (1993), with little of the usual terminological intricacies of current theoretical writing, straightforwardly poses the intriguing comparative question of why it is that all archaeological theoretical approaches to date have served to make it unsurprising to discover that the world had already been populated by humans long before European

voyages of enquiry. He then goes on to suggest a comparative research programme to rectify the situation.

Rowlands (1994:138) warns against the dangers and 'limitations of the archaeological project on a global scale'—seeing such global approaches as having forced local archaeologically attested trajectories to be evaluated within some western-defined 'benchmark in human progress such as the origins of farming, or of metallurgy…'. In its place he foresees:

> thousands of local archaeology societies producing their own accounts of local pasts simultaneously on a global scale…. If the European experience is anything to go by, this is what follows from a successful creation of a sense of nationhood usually in the guise of the creation of a national museum followed by local museums to re-present subordinated identities.

Who could not prefer the Gamble scenario, with its seemingly theoretically unloaded question? As I understand him (and unlike the earlier approach of Grahame Clark), *all* his global timewalkers are, by definition, equal before the world—the research project taking as its benchmark the successful achievement of 'voyages of discovery', *whatever the date* at which they occurred. Furthermore (Gamble 1993:48), and incidental to the initial comparative question posed, such a global research perspective will have the additional positive result that 'archaeologists will have to reflect on the history of the subject which informed scientific activity and resulted in theory-laden concepts of time, humanity, colonization and centres of origins'—theory-laden concepts based on a series of questions of questionable value in today's contexts.

There is little doubt that many archaeologists believe that the strength of Childe (see above) was in his development of theory that allowed interesting global comparisons—even if the comparisons that he highlighted may not be of the same kind that would be of interest today (and see Sherratt 1993:126). In any redefinition of the comparative method, it would be possible to differentiate between comparison based upon similarity—seeking to homogenize humanity—and comparison based upon contrast—seeking to use ethno—and local history to underline the specificity of the archaeological case (Julian Thomas, pers. comm.).

We are currently in a situation described succinctly by Rowlands (1994:139): 'In the shift from the modern to the postmodern we witness the replacement of angst about the alienation of the subject by the fragmentation of the self'; i.e., archaeological theory today is 'metaphysical' speculation which can be distorted by interests such as nationalism, and which therefore needs an internal objectivity. Yoffee & Sherratt (1993:7) go further than this, forecasting the end of the archaeological discipline if archaeologists do not put a stop to the number of proliferating multiple versions of the past (and see above) and, above all, if they claim to be the 'guardians of its integrity'. Yet, they forget that earlier archaeological theory allowed Phoenicians to reach Great Zimbabwe, as well as

Tara in Ireland, and ancient Greeks and Egyptians to reach Namibia, and few (if any) post-processualists have more difficulty in denying the accuracy of such earlier interpretations than do processualists, or whoever. A post-processual approach is perfectly able to make statements about the relative merits of alternate hypotheses (and see Stark 1993:98–9, who also raises the question as to whether the western opposition between 'subjective' versus 'objective' interpretation is necessarily an apt one). Indeed, as claimed above, the existence of competing theory is a strength if the aim is no longer to exclude others by insistence that only one dominant interpretive paradigm (proposed, and acceptable, for reasons of gender, politics and/or power) is to be heard.

What has become clear is that Yoffee & Sherratt (1993), as others in the same book, have set up a straw man: 'the branch of post-proccessualism that argues that there are multiple versions of the past and that all or many of them might be equally valid…' (Yoffee & Sherratt 1993:7). For although I (e.g., Ucko 1994b: 270) and many others have argued that much archaeological interpretation is 'subjective', and however much those with whom I have walked in Whipsnade (see above) believe in the vesting of emic significance on, or in, the remains of the past, we have no real difficulty in at least saying that at the moment there is no evidence to support the mass media in its view that dinosaurs and human beings enjoyed some sort of intercourse together! And, indeed, given the appropriate socio-political context, any post-processualist could march in protest with the best of the culture-historians against whatever manipulation of such figmented intercourse was being fed to the public (or, e.g., 'Boxgrove Man' 's role in facilitating 'any Englishman's [entitlement to] walk a little taller in the recognition that he is descended from such a striking creature' (*The Times,* 18/5/94)). As Hodder also says: 'We do not need, as archaeologists, to feel that the only alternative to positivist processual archaeology, is a hopeless slide towards relativity and chaos' (Hodder 1991b:21–2), or, as Said (1983:241–2) put it in a non-archaeological context:

> [critical consciousness] is a sort of spatial sense, a sort of measuring faculty for locating or situating theory, and this means that theory has to be grasped in the place and the time out of which it emerges as part of that time, working in and for it, responding to it; then, consequently, the first place can be measured against subsequent places where the theory turns up for use.

Thomas (Ch. 17:353) stresses the point that the existence of competing hypotheses does not in itself in any way necessarily imply that there is no reality to be discovered (any more than does the co-existence of physics and cosmology). In promoting the concept of 'perspectivism', and with reference to Hodder, he says:

> What this implies is that there is an external reality, which is both hugely complex and spread across enormous periods of time. Human beings will only ever experience a fragment of this totality, and their attempts to encompass even a small part of the whole in knowledge and language are gross simplifications. Thus there was also a real past, and we do experience real traces of past persons, yet our understanding of that past must be one which is imperfect. Perspectivism implies that while reality exists *out there,* we apprehend it from a perspective, and that our understanding will be one amongst many. Consequently, our knowledge is always incomplete, there is always more to know about the past and the present, and there will be no final point at which we have achieved a definitive understanding.

Encouragingly, although it does not seek to arbitrate between competing claims and counter claims, such an approach allows respect for non-archaeological versions of the past, as also being incomplete understandings of what may have gone before. This is a very different approach from the former assumption that cultural/imperial domination somehow entitled an archaeological past to be forced on to unwilling peoples who were not interested in archaeological findings because they had their own theories about the past (e.g., Ucko 1983; Ucko 1994b). Emotional commitment to such non-archaeological pasts is *at least* as strong, and therefore *as valid,* as any vested by those involved in nationalisms, regionalisms, or whatever, via a culture-historical, processual or post-processual archaeological past. To accept this makes possible the formulation of challenging new theoretical paradigms to further understand the nature of past human endeavours. As Rowlands (1994:130) has put it, in the context of analysing the events of the 1986 World Archaeological Congress: 'What was striking about this challenge to archaeological naivety was the role of non-European archaeologies in challenging the metanarratives of principally European—and North-American-dominated global archaeology'.

With such a new approach to the question of empiricism and the nature of that part of the past which is uncovered by archaeologists, as well as by others, it will be fascinating to see whether most of the world's (non Anglo-American) archaeologists will continue either to reject all archaeological *theory,* or continue to accept only a culture-historical framework of interpretation—or whether, instead, new trends will now arise. As we have already seen (Moser, Ch. 7), the encouraging message from Australia is that recognition of Aboriginal participation in the creation of the past has already revealed a shared concern with archaeologists of other countries, such as the United Kingdom, and those involved in Unesco's World Heritage listings, to redefine areas of archaeological significance from site to cultural landscape, a redefinition not based on any theoretical ecological model but on the recognition that such definitions must encompass the realms of human perception and cognition. Australia is not alone in coming (willingly or not) to share its archaeological and anthropological aspirations with indigenous peoples:

> Anthropologists are increasingly being summoned to work for the Indians as their interpreters to the national powers. Topics of research are less and less the exclusive interest of the ethnographer. From the moment the Indian asks you what you will do with all those questions and answers, they are making you accountable for your presence amongst them. They will then file you for future reference. What the profession will do with this is still to be seen.
>
> (Ramos 1994:86)

If all but the most restrictive 'theory' is rejected by the archaeological world, we may indeed be about to witness the demise of the discipline. On the other hand, if 'theory' is to be a self-consciously aware but welcoming approach to a diversity of approaches to the past, we may be about to take part in an archaeological development which will reveal how archaeological investigation and interpretation can add a new dimension to the world's understanding of itself.

> Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist.
>
> (Keynes [1936] 1992, p. 383)

> Those who do not 'theorize' (i.e., those who think that theorizing is a waste of time, a luxury for the idle) and therefore choose to employ 'common sense' theories in their practices, are of necessity people who excavate badly, who fail to publish (or who publish badly) and who present a commonplace and dull synthesis of the past.
>
> (Jorge & Jorge, Ch. 11)

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CHAPTER ONE
GREAT ZIMBABWE AND THE LOST CITY
The cultural colonization of the South African past

MARTIN HALL

In the South African 'homeland' of Bophuthatswana the international hotel group Sun International has constructed an archaeological site (Figs 1.1–4). The Lost City is modelled on a ruin, imagined as destroyed three thousand years ago. The architecture of this resort is a study in post-modernist image play. Behind the design is a myth, concocted by the Lost City's California-based design team, which has a nomadic tribe journeying from northern Africa to a secluded valley in modern-day Bophuthatswana. Steadily, they built a rich civilization, only to have their city destroyed by an earthquake. Three thousand years later this lost valley was discovered by an explorer who vowed to restore it to its original splendour. Gerald Allison, the principal architect, claims that the myth, like the City, is pure fantasy, although 'colored by the heritage of Africa' (pers. comm. 1992). But elsewhere I have argued that there is nothing new about the myth at all—it is simply a modern rendering of a European notion of Africa that can be traced back through many centuries of European fantasy (Hall 1993).

Allison's myth, like many before it, has a tripartite structure of early civilization, destruction by a dark force, and rediscovery by enlightened adventurers. Africa is perpetuated as 'the heart of darkness'—the canvas for Europe's fantasies of domination and control.

At first sight the history of archaeology in Southern Africa—and the application of European archaeological theory in the subcontinent—may seem like an enlightened counter-balance to such old colonial stories. And, of course, archaeological research has opened up a new dimension to African history, demonstrating both the antiquity of 'Iron Age' farming settlement and the diversity and complexity of culture and economy (Hall 1990). But critical inspection shows that this 'new history' has also become bound up in the popular mythologies which continue to depict Africa in time-honoured paradigms. This is hardly surprising; archaeologists, as much as everyone else, must surely be seen as saturated in the ideologies of their times.

GREAT ZIMBABWE

One important intersection between early popular mythology and archaeological practice came with the 'discovery' of Great Zimbabwe more than a century ago



Figure 1.1 Architect's model, Palace of the Lost City (publicity release, courtesy of Sun International).

(Figs 1.5–6). Ever since European settlement in southern Africa at the very beginning of the sixteenth century, expeditions had been mounted to search out the fabled wealth of the lost civilizations of the continent's interior. These invariably ended in frustration, and it is testimony to the power of the legend of lost cities that the search continued at all.

But in 1871 tangible evidence at last came to light. Carl Mauch, an energetic and credulous explorer, came across the ruins of Great Zimbabwe[1]. Mauch took



Figure 1.2 Carvings and statues are found throughout the Lost City: Leopard crouching on a rock 'guarding' the Temple of Creation' (Photo: Sue Diamond)

up a posture that Hollywood was to adopt for its image of the archaeologist a hundred years later. Working alone, with sketch-book and revolver at the ready, he struggled 'through thick grass intertwined with leguminous creepers' while keeping 'well hidden from possible observers by the tall grass'. Applying a rather tenuous chain of reasoning, Mauch noted that splinters of wood from a cross-beam were very similar to the wood of his pencil, indicating that both were

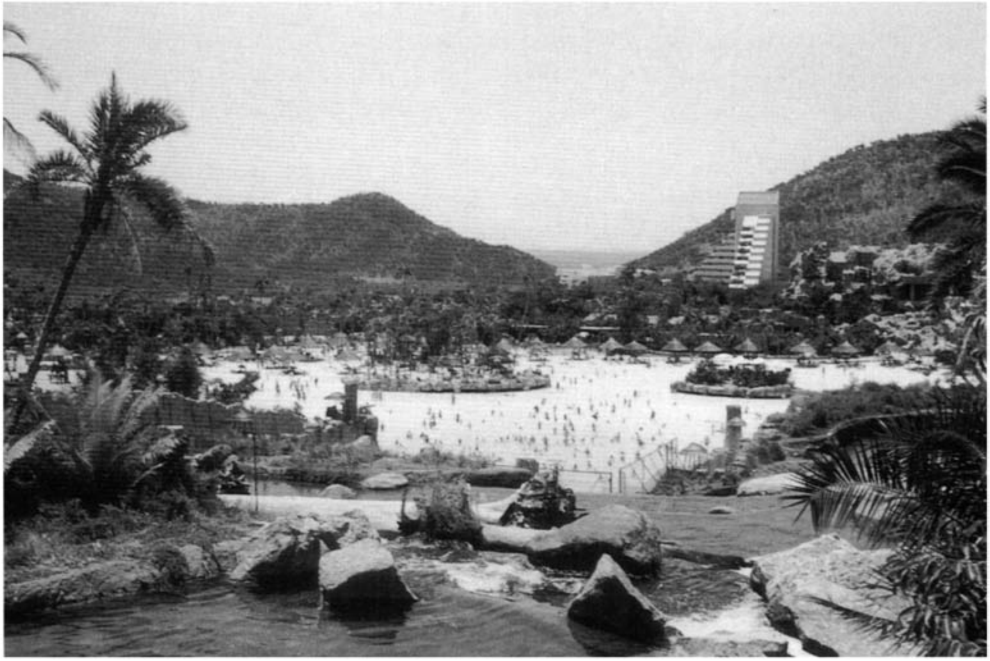


Figure 1.3 The Valley of Waves' (artificial beachpool) viewed from the 'Palace' (Photo: Sue Diamond)

cedar. This was conclusive: 'one gets the result that the great woman who built the *rondeau* could have been none other than the Queen of Seba *(sic)*' (Burke 1969:190) (Fig. 1.6).[2]

For the first time the old ideas of the Lost City could be positioned on a map and represented by specific architecture. Appropriately, Thomas Baines's 'Map of the Gold Fields of South Eastern Africa', published in 1873, showed the ruins of Great Zimbabwe and labelled them as 'the supposed realm of Queen of Sheba' (Etherington 1984). In turn, such geographical specificity gave new impetus to popular belief. One of the beneficiaries was Henry Rider Haggard, in South Africa between 1875 and 1881, who found commercial success in 1885 with his third novel, *King Solomon's Mines*. Inspired by the newly discovered Mashonaland ruins, Rider Haggard offered his readers a powerful metaphor of Africa as a dark sea of barbarism, representing the dark side of the personality, and masking the radiance of long lost civilizations buried deep in the past (Haggard 1885).

Given such a scenario, who could doubt that the Imperial mission was a just cause (Etherington 1984)? In 1890, Mashonaland was occupied by the British South Africa Company and Great Zimbabwe became an Imperial possession. Cecil Rhodes, the driving force behind British colonialism in southern Africa at this time, became obsessed with the place, acquiring Mauch's finds and



Figure 1.4 The 'Bridge of Time' with the 'Palace' in the background (Photo: Sue Diamond)

equipping expeditions to dig for evidence of Phoenician settlement. Together with the Royal Geographic Society and the British Association for the Advancement of Science, Rhodes sponsored the first archaeological expedition to Great Zimbabwe, led by Theodore Bent, an antiquarian who had travelled extensively in the eastern Mediterranean and who was considered an expert on Phoenicia (Garlake 1973). The results of Bent's excavations were published in 1892 under the title 'The Ruined Cities of Mashonaland'; a combination of archaeological report (with specialist sections on astronomical implications by R.M.W.Swan) and Victorian travelogue (Bent 1892).

Bent dismissed popular opinion and asserted the need for proper scientific work if the truth was to be found. The suggestion that Mashonaland was a lost Biblical land did not 'satisfy the more critical investigation to which subjects of this kind are submitted in the present day' (Bent 1892:228). Coming as it did from an expert on the Phoenicians, this seemed a heavy blow against the popular belief that Rider Haggard had so widely popularized six years earlier. But although Bent dismissed King Solomon and the Queen of Sheba from his stage, he retained and strengthened the structure of the mythology that had sustained them for so many years. Bent used his authority as an orientalist, buttressed by the expert evidence of Professor Muller of Vienna, 'the great Austrian authority on Southern Arabian archaeology', to show that Great Zimbabwe was the work

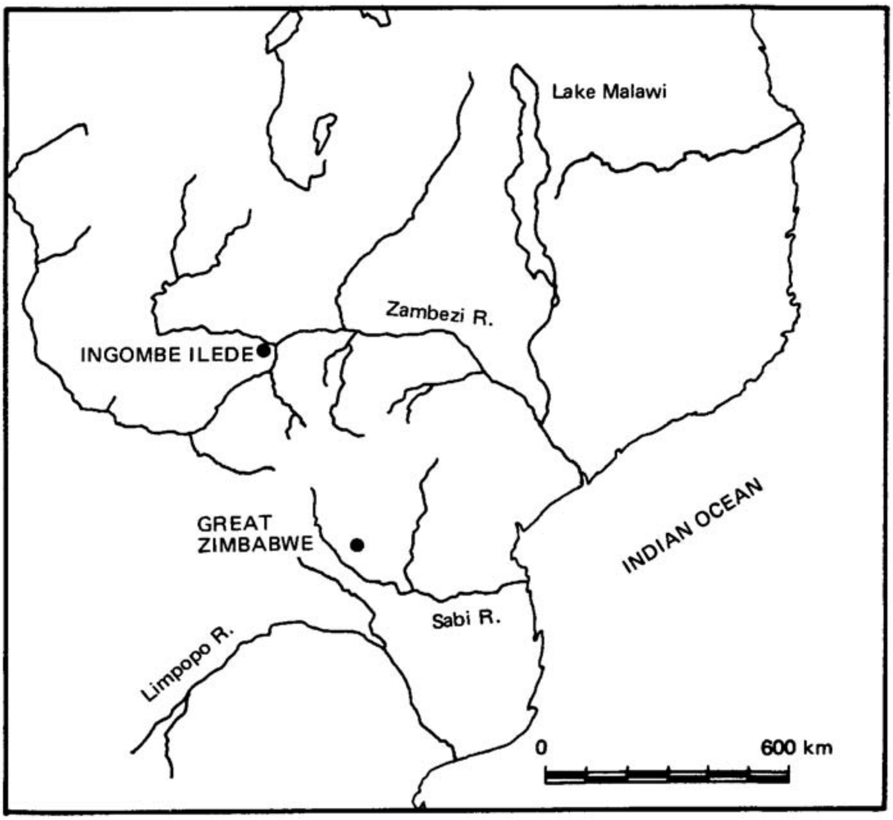


Figure 1.5 Great Zimbabwe is located on the southern edge of the Zimbabwe Plateau, above the Sabi river. Ingombe llede, on the Zambezi river to the north, was a major point of trade connection between the Zimbabwe state, people to the north, and Indian Ocean traders (see Hall 1990).

of Sabaean Arabs, garrisoned in enemy territory and mining gold for Mediterranean and Asian merchants.[3] The influence of Phoenicia was marked by the evidence for phallic worship, while the 'Temple' served as a solar calendar (Swan, in Bent 1892:174). This example of early civilization was still destroyed by a 'dark force'—the ancestors of the local 'Karanga' whose past, 'like all Kaffir combinations', was a story of 'a hopeless state of disintegration' (Bent 1892:33, 43).

Theodore Bent claimed Africa for the archaeologist, 'almost the very last person who a short time ago would have thought of penetrating its vast interior' (Bent 1892:42). It was also clear that some of the artefacts from sites such as Great Zimbabwe could have commercial value, and Rhodes moved to turn antiquarianism to profit. He established Rhodesia Ancient Ruins Ltd as a company with exclusive rights to work the sites for treasures. Involvement in this licensed depredation gave a local journalist, Richard Nicklin Hall, the



Figure 1.6 The Great Enclosure at Great Zimbabwe—fantasy palace for the Queen of Sheba, now interpreted as either the royal residence or a female initiation school (Photo: author)

opportunity to present himself as an antiquarian and scholar. His *The Ancient Ruins of Rhodesia,* published in 1902 in collaboration with W.G.Neal (a local prospector) offered an archaeological periodization; a 'Sabaean Period' (2000–1100 BC) followed by a 'Phoenician Period' (from the start of the Christian era), a transitional period, and a final 'decadent period', as descendants of the first builders mixed with the local population (Hall & Neal 1902).

In 1902 Hall was appointed curator of Great Zimbabwe; he dug the site extensively, and published his results in a second book (Hall 1905). He closely followed Theodore Bent's interpretations of the site—writing of phallic worship, worship of the sun and the moon and the destructive force of the 'Kaffirs'—and also reflected the beliefs of his times. The list of subscribers to his later *Pre-historic Rhodesia* included the pre-eminent names of the southern African colonies (Hall 1909).

Hall's enthusiasm was his undoing. He dug trenches with a casualness which showed none of Bent's declared respect for method, and wrote of his 'lost city' with none of the dry caution of the scholar. It was hardly surprising that such an approach should attract a backlash—and this came in the British Association for

the Advancement of Science's decision to support a second expedition to Great Zimbabwe, to be lead by David Randall-MacIver, a pupil and colleague of Flinders Petrie.

Randall-MacIver excavated at Great Zimbabwe in 1905 and published his results in the following year (Randall-MacIver 1906). He has been recognized as the founder of modern fieldwork at the site—in Peter Garlake's assessment, Randall-MacIver's 'approach had been faultless, his excavations careful and his assessment of the basic culture of the occupants of Great Zimbabwe unassailable' (Garlake 1973:78). But although Randall-MacIver rejected amateur, Biblical interpretations of the site, and argued that Great Zimbabwe was part of Southern Rhodesia's ethnographic record, his writing was based on the same racial assumptions that ran through the conclusions of his predecessors. Thus he made his case not by elevating the 'Makalanga' to a higher cultural status, but by bringing the workmanship of Great Zimbabwe down to the native level. 'The building, fine as it is', he wrote,

> has been executed in exactly the same spirit as all the other 'ancient monuments' in Rhodesia. Laborious care has been expended on the most conspicuous and effective parts, but elsewhere the workmanship is slipshod. Probably several gangs were engaged on different parts of the wall at the same time, and, like clumsy engineers boring a tunnel from different ends, they failed to meet at the agreed point of junction.
>
> (Randall-MacIver 1906:68)

Randall-MacIver's fieldwork fuelled rather than settled the controversy that Hall had set up. Rhodesia's small, noisy settler community could hardly accept that one of the basic tenets of their new history could be swept away by an outside 'expert' (Garlake 1982a). Eventually, in 1929, the British Association for the Advancement of Science commissioned a third archaeological investigation of Great Zimbabwe, this time by Gertrude Caton-Thompson.

Like Randall-MacIver, Caton-Thompson had had previous experience excavating in Egypt. And, also like Randall-MacIver, she has been credited with a formative status in modern archaeological work in southern Africa. In my own overview of the archaeology of southern Africa's farming communities, I wrote:

> In her approach to the archaeological problem, Caton-Thompson was systematic and professional. She ignored the temptation to range widely across the rich archaeology of the region, and concentrated on Great Zimbabwe itself and a small set of nearby stone ruins…. By carefully excavating deposits, Caton-Thompson was able to classify pottery by its colour, texture and finish—a standard archaeological technique then, as today.
>
> (Hall 1990:7)

Caton-Thompson would have agreed with this assessment, claiming in her own writing to be free of all prejudice as to Great Zimbabwe's origins and the culture of its builders, and mindful of the need to avoid the 'wildernesses of deductive error'. She stressed the need for systematic empiricism resting on a foundation of stratigraphy and unequivocal chronological evidence (Caton-Thompson 1931:2).

Caton-Thompson's conclusions, remembered half a century later, ring out as a clear manifesto for African history. Reporting to the British Association for the Advancement of Science in Johannesburg in August 1929, she argued that

> instead of a degenerate offshoot of a higher Oriental civilization, you have here a native civilization unsuspected by all but a few students, showing national organisation of a high kind, originality and amazing industry. It is a subject worthy of all the research South Africa can give it. South African students must be bred to pursue it.
>
> (Caton-Thompson 1983:132)

Most responses were favourable and Abbé Breuil presented an authoritative accolade, declaring that 'la question est absolument finie' (although Raymond Dart, notorious for his racism, made a loud protest; Caton-Thompson 1983).

However, as with Randall-MacIver, there is an underside to Caton-Thompson's work that does not conform comfortably with this enlightened image. For, despite her claims to be free of prejudice, she could not slough off the assumption that Africa could not be 'civilized':

> If by indigenous we mean an origin born of the country on which they stand, then the ruins are, in my opinion, indigenous, in a full sense of the term; though at an epoch in the world's history as late as that to which we date the ruins, no one would deny the possibility—rather they would urge the inevitability—of exotic stimuli, physical and cultural, received and absorbed during ages of coastal contact with alien trading peoples.
>
> (Caton-Thompson 1931:7)

Racial assumptions permeate her reading of that most empirical of evidence, the archaeological section. In discussing the stratigraphy of her excavations, she interprets changes in colour and texture of the soil as the impress of two 'periods', reflecting in turn the passage through the site of two 'peoples', or 'cultures': the 'Zimbabwe Period' of the 'Zimbabwe Culture' and the 'Daga Period' of the 'Daga People'. Caton-Thompson follows Randall-MacIver in seeing this Zimbabwe Culture as not up to much:

> The architecture at Zimbabwe, imitative apparently of a daub prototype, strikes me as essentially the product of an infantile mind, a pre-logical mind, a mind which having discovered the way of making or doing a thing goes on childishly repeating the performance regardless of incongruity.

(Caton-Thompson 1931:103)

Inevitably, as with all things African, the trend is downhill—'unbroken though retrogressive continuity of custom down the ages since Zimbabwe was erected… away from the best towards deterioration…' (Caton-Thompson 1931:57). At some stage, though, the 'Daga People' arrived, further 'debasing' the 'amenities of life'.

Caton-Thompson found an ethnographic analogy for this transition in 'the savage Zulu conquests of the early nineteenth century', again belying her claims to have an open mind. She wrote:

> We hardly need, I think, rack our brains to find reasons for the decline of the Zimbabwe culture from its zenith, in, say, the tenth or eleventh centuries, to the lowlier condition in which the Portuguese describe it (though at second-hand) some five hundred years later, when it still bore the stamp of its high descent. The migratory Bantu hordes, which within a short span of the Portuguese records like locusts appear, devastate, and disappear from history, must have had forerunners in earlier times.
>
> (Caton-Thompson 1931:192)[4]

But if Africa was condemned to inevitable barbarism, how could Great Zimbabwe be accounted for at all? First, as the work of a savage genius—a medieval Shaka—an 'autocratic master mind, stamping his individuality upon the herd' (Caton-Thompson 1931:195). And second, as the result of the 'imitative talent of the Bantu'. 'Is it outside the range of possibility', Caton-Thompson asks,

> that a minaret of an early mosque in one of the coastal settlements, Persian or Arab, gave the idea for the Conical Tower, easily enough executed by natives, whose whole building talent follows circles and curves, and who, if my conception of their central African origin is correct, were probably already heirs to some primitive sacrificial ceremony in which a conical mound played a part? And though I am unable to admit direct racial derivation from Arabia or Mesopotamia, have we, by a long process of typological derivation, found in the Mohammedan minaret the connecting link between the Zimbabwe cone and its ancient Semitic prototype, so strongly urged by many inquirers, but so impossible chronologically?
>
> (Caton-Thompson 1931:101)

Gertrude Caton-Thompson's medieval ruin was not the same place as Bent and Hall's Lost City; there was little wealth, not a great deal more than a typical 'Bantu kraal' in the buildings, and no hint of paradise. But the critical influence from Old World civilization was still there—via the Arab trading cities of the east African coast—and the heart of Africa was still as dark and threatening as it ever had been.

Only a limited amount of excavation followed Caton-Thompson's 1929 season —most notably by Roger Summers and Keith Robinson in 1958. Most of this research was concerned with verifying stratigraphic sequences and establishing a radiocarbon chronology.[5] Its value lay in establishing and testing the empirical foundation upon which modern interpretations have come to rest.

The town of Great Zimbabwe is now generally seen as consisting of three parts: the buildings on and around the granite hill which rises above the north side of an open central court *(dare),* the structures to the south of the court, and the surrounding town (Fig. 1.7). These parts comprise an integrated whole.

The hill ruins (the 'Hill'—Bent and Hall's 'Acropolis') consist of lengths of stone walling running over and between large granite boulders. These form a set of enclosures, in some of which were once substantial accumulations of archaeological deposits. Excavation has shown that these hill walls sheltered plaster and timber houses, often rebuilt (Robinson 1961).

The court is an open area. To its south are more stone enclosures, free-standing in this part of the town, the largest of which is the 'Great Enclosure' (the 'Temple' in earlier romantic imagination). The Great Enclosure has particularly massive and elaborate walling and surrounds a number of smaller enclosures and other architectural features. Although the deposits had been extensively damaged by early depredations (particularly by R.N.Hall), careful excavation by Roger Summers, linked with an interpretation of the architecture of the walling offered by Anthony Whitty, has provided a stratigraphical sequence to match Robinson's work on the Hill (Summers 1961; Whitty 1961). With the exception of Caton-Thompson's area excavation of one other, complete enclosure south of the court (Caton-Thompson 1931), the other stonework in the valley has been little investigated.

The surrounding town must have been substantial—perhaps housing as many as 30,000 people (Huffman 1986). But apart from one excavation, not fully published, the archaeology of the areas where most of Great Zimbabwe's population lived remains unknown (Huffman 1977).

Robinson, Summers and Whitty were restrained in their interpretations, although they certainly made assumptions about what the empirical evidence could mean (their interpretations are discussed in detail by Garlake 1973). Peter Garlake and Thomas Huffman, on the other hand, have offered detailed, and competing, interpretations of Great Zimbabwe's architecture which well illustrate the application of archaeological theory to an African problem in more recent years (Garlake 1973; Huffman 1981; Huffman 1982; Garlake 1982b; Huffman 1984).

Huffman and Garlake agree that Great Zimbabwe's walls were not an integral part of houses in the town, rather serving to shelter some areas from public view, emphasizing status and power. There is also broad agreement on the status of the town on the wider stage of southern Africa, although Garlake argues for a number of regional centres of importance along the edge of the Zimbabwe

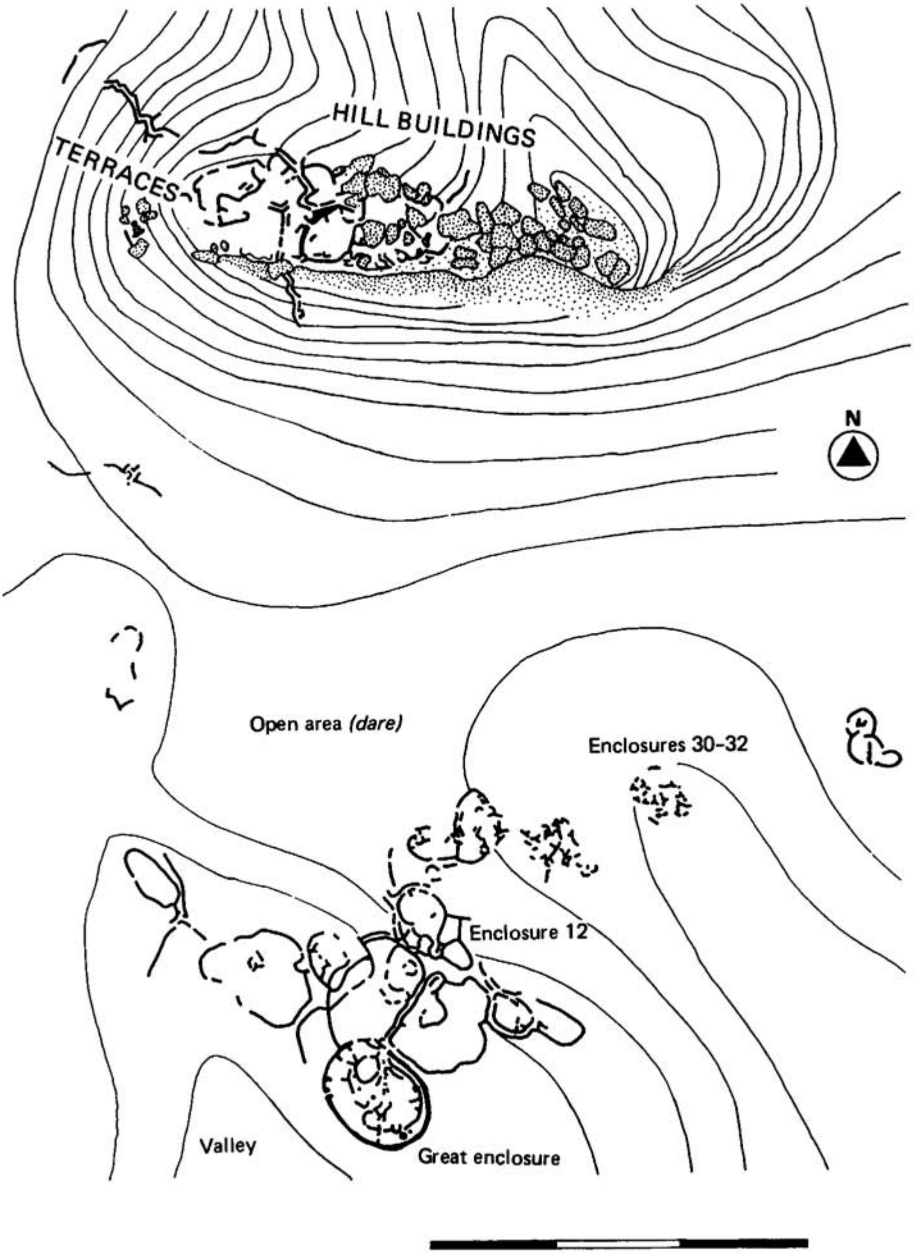


Figure 1.7 The central precinct of the town of Great Zimbabwe. The bulk of the town, where ordinary people lived, surrounded the stone buildings; these outer areas have been little excavated (see Hall 1990)

Plateau, while Huffman believes that the size of Great Zimbabwe, and in

particular the size of its central court, indicates its unequivocal pre-eminence.

But here the agreement ceases. Garlake sees the buildings that would have stood within the enclosures south of the central court as the houses of a ruling class. Huffman, on the other hand, argues that the numerous grooved slots in these buildings were female symbols, designating the houses of the royal wives (Fig. 1.8). One particular enclosure (known variously as the 'Renders Ruin', after an early explorer, and 'Enclosure 12') Garlake sees as the royal treasury, so identified by the rich hoard of imported goods found there in 1902. Huffman, on the other hand, feels that such a cache must have been associated with the king's first wife.

Huffman and Garlake also disagree about the place where the ruler himself lived. Huffman is convinced that the Western Enclosure on the Hill, with its substantial accumulations of occupation debris, was the residence and audience chamber of the king, and that the secluded Eastern Enclosure, at the other end of the Hill, was a religious centre. Garlake agrees that the Eastern Enclosure had a spiritual role, and also that the king must have lived on the Hill in Great Zimbabwe's earlier years. But Garlake sees the Great Enclosure, south of the central court, as the king's residence through the later periods of occupation, with its towering outer wall emphasizing status. Once the king had moved down to his new residence, Garlake argues, the buildings on the Hill became the domain of those who could control the spirit world.

Huffman interprets the Great Enclosure very differently. Taking as his analogy Venda initiation schools for girls (recorded ethnographically in the Transvaal), he suggests that women who lived in the southern part of the town used the buildings within the Great Enclosure during their instruction of the daughters of ruling families. Huffman interprets architectural features of the Great Enclosure as symbolic aids in this process of instruction: the giant conical tower standing for senior male status, the smaller tower expressing the role of senior women, and designs in the walling representing male virility and female fertility.

In their interpretations of Great Zimbabwe, both Garlake and Huffman mobilize Africa's ethnographic record—Garlake writes about the relationship between rulers and spirit mediums, and Huffman turns to Shona ethnography to understand the connections between heaven and earth and the role of the first wife in guarding her husband's possessions, and to Venda ethnography in interpreting the Great Enclosure as an initiation school. The differences in Huffman and Garlake's readings of the site, on the other hand, stem from the application of different strands of European social theory.

Garlake offers a prosaic interpretation of the town—one that would fit early towns in many parts of the world. The king is secluded behind the high walls of the Great Enclosure, surrounded by the residences of the principal courtiers—a ruling class. The spirit medium—the main agent of religion—is separated, with his domain on the Hill, suggesting a tension between secular and sacred authority. Ordinary people—the underclass—live in the area surrounding this city centre. Although this interpretation is tinted by an acknowledgement of its

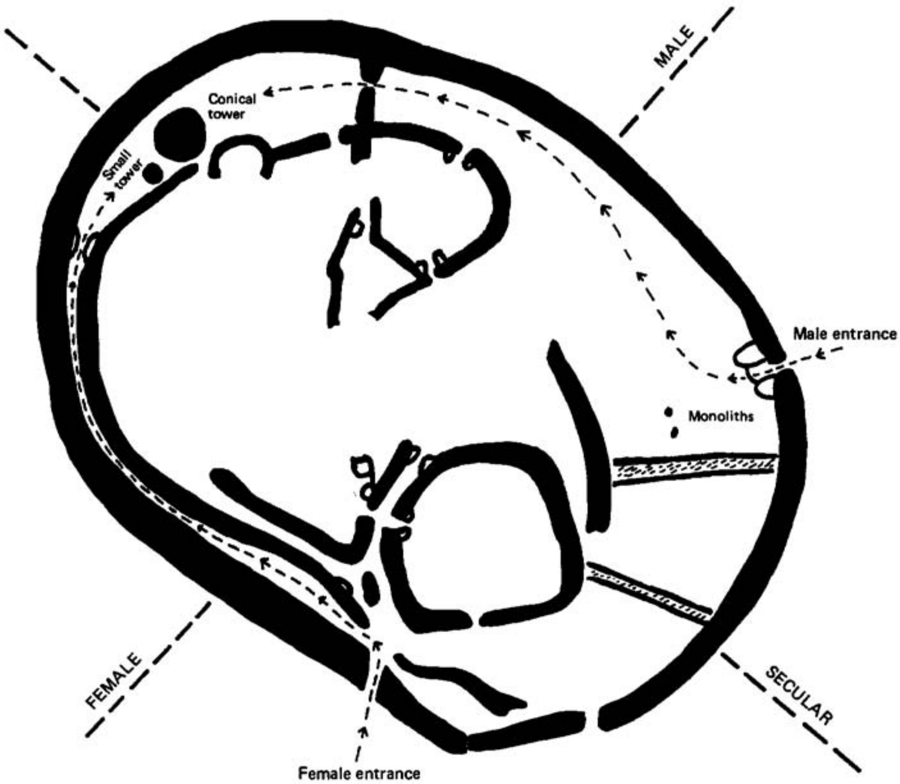


Figure 1.8 A structuralist interpretation of the Great Enclosure, Great Zimbabwe. Architectural features are read as pairs of oppositions along the axes of male-female and secular-sacred (see Huffman 1981)

African context, it fits well into the broad tradition of economic interpretations of city design and function.

Huffman's, in contrast, is a structuralist reading, based directly on Kuper's earlier interpretation of settlement patterns in southern Africa (Kuper 1980; Fig. 8). Huffman understands Great Zimbabwe along an axis of gender—the 'male' hill in opposition to the 'female' valley. Both domains are marked out by architectural features, understood as gendered symbols: 'male' monoliths and Hill paired and opposed to 'female' grooves and Valley.

One of the sharp ironies of Great Zimbabwe is that the extent of damage done to the archaeological deposits by licensed vandals such as R.N.Hall has made it very difficult to resolve interpretative differences through fieldwork. So much of the deposit on the Hill has been shovelled over the side of the precipice, for example, that there is very little left to excavate.

A second irony is that, although the structuralist interpretation is by far the more comprehensive in that it accounts in a coherent manner for large sets of

architectural details and artefactual evidence, it also freezes Great Zimbabwe as part of a timeless, ahistorical mindset. In seeing Great Zimbabwe as a consequence of a widespread, long-lasting 'cognitive pattern' (a structuring way of seeing all aspects of the world), Huffman is saying that the 'Bantu' view of the world is the same today (in Venda and Shona ethnography) as it was a millennium ago (when, he argues, the cognitive pattern was first manifested at Mapungubwe). Is this too close for comfort to the earlier assumption that Africans were incapable of change, that there could be no African history and that, *ipso facto,* Great Zimbabwe must have been built by outsiders?

AFRICA'S LOST CITY

The old legend of Africa's Lost City remains secure in the mass system of circulation of modern popular culture. Wilbur Smith's immensely popular novel *Sunbird,* a best-seller for twenty years, projects a Carthaginian paradise on to the dry wastes of Botswana, and compares the destruction of this Lost City of Opet by the southward-migrating Bantu hordes with the threat to white civilization in Africa by communist-led black nationalists (Smith 1972). In contrast, the latest Hollywood rendering of *King Solomon's Mines* is packaged as light entertainment and combines slapstick and special effects with a fast-moving story. But the old mythology is repeated without challenge. At the heart of Africa is the Lost City of the Queen of Sheba and her fabulous diamond mines; the local natives are absurdly barbaric and their fate is in the hands of enlightened European explorers.

Sunbird and *King Solomon's Mines* have gained their success—measurable directly in sales reports and box office returns—not from the patronage of a gin-sodden colonial residue, living out the last days of white supremacy in a haze of nostalgia, but from North European and North American readers and audiences who would prefer to pay for an image of Africa as the time-honoured dark continent. Sun International's planners have realized this in their promotion of Bophuthatswana's Lost City, which is pitched explicitly for North European and North American tourists. The new hotel (the 'Palace of the Lost City') and theme park take the old appeal of the popular romance into the extravagant multimedia environment of late capitalism: 338 rooms, restaurants with cuisines from around the word, slot machines, cinemas, and a massive water park. The special effects are billed as 'imported from Hollywood', enough to 'make even Indiana Jones feel at home' (*Sunday Times* (Johannesburg), 4 October 1992).

European archaeological theory—brought to bear on the problem of interpreting Great Zimbabwe—has had an ambiguous relationship with such popular representations of Africa, as expressed in legends such as that of the 'lost city'. Archaeologists including Bent, MacIver and Caton-Thompson proclaimed themselves to be detached empiricists, letting the evidence 'speak for itself' and deriding Biblical explanations. But their empiricism did nothing to prevent the racism that underlay the European colonial enterprise in Africa from

saturating their archaeological interpretations of Great Zimbabwe. Later archaeologists, working as the 'winds of change' began to undermine colonialist assumptions, eschewed such racism. However, in seeking the maximum distance from bush-happy predators of the likes of Bent and Hall, 'modern' archaeology has instated MacIver and Caton-Thompson as founding figures of the discipline in southern Africa.[6] In consequence, these writers' assumptions of African ineptitude and the inevitability of foreign contact remain woven into the fabric of Great Zimbabwe's interpretations. Novelists, Hollywood's script writers and the corporate designers of holiday resorts can turn to recently published works that claim academic respectability, and which continue to present Africa as the 'heart of darkness' (e.g., Gayre 1973; Hromnik 1981; Mallows 1984. A book which perpetuates all these myths, but from the starting-point of an idiosyncratic African nationalism, is Mufuka 1983).

What are the implications of this history of interpretation for archaeological practice in southern Africa? Overall, the possibilities of significantly changing racist representations of Africa's past in European and North American culture may be slim. Thousands more will be influenced—through the power of mass media—by stories of Phoenicians, lost white civilizations and African barbarism than will be influenced by archaeological writing. Post-modernism—which celebrates pastiche and the collapse of the historical dimension—seems to be giving such fantasies about Africa new respectability and marketability.

But, on the other hand, it is depressingly fatalistic to surrender in the face of the inevitable success of Bophuthatswana's Lost City, King Solomon's Mines and the *Sunbird*. Any challenge to archaeology's collusion in racist representations (deliberate or inadvertent) of Africa's past must depend on continuing critical assessment. My point is not that European theory should be inadmissible in the archaeology of places such as Great Zimbabwe, but that those often-hidden assumptions and implications that may be imported with such theory should be carefully exposed for what they are.

NOTES

1 Mauch set out on his expedition already convinced that the area contained Solomon's Ophir, having been influenced by Rev. A.Merensky, long a missionary in the Transvaal. Merensky wrote to the *Transvaal Argus* on 12 October 1868 that 'in the country Northeast and East of Mosilikatse the ancient Ophir is to be found and that in the times of the Ptolomies Egyptian trade penetrated to our coasts' (Quoted in Burke 1969:4).

2 The wood Mauch was describing was probably *Spirostachys africana,* indigenous to the region; Garlake 1973.

3 Muller was evoked by Bent in the prefaces of the second and third editions of his work, published in 1893 and 1895.

4 Caton-Thompson's reference to the 'short span of the Portuguese records' is an allusion to the 'Bantu Chronology' of A.T.Bryant, whose *Olden Times in Zululand*

and Natal was hot off the press when Caton—Thompson was writing her book (Bryant 1929). Bryant's chronology and most of his interpretations have been shown to be without foundation.

5 This fieldwork has been reviewed critically by Garlake (1973). Robinson, in particular, followed Caton-Thompson's ideas of tribal invasions, establishing a sequence of 'periods' marked by implications of military action, victory and defeat (Robinson 1961).

6 Garlake (1973) goes to particular trouble to preserve Caton—Thompson's own image as an impartial interpreter by modifying a quotation, thus disguising Caton-Thompson's racism (see Hall 1993).

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CHAPTER TWO
'THE HUN IS A METHODICAL CHAP'
Reflections on the German tradition of pre- and proto-history

HEINRICH HÄRKE

Stereotypes are often misleading, but more often than not, they also contain some element of truth, however much distorted. The stereotypical view that 'The Hun is a methodical chap', expressed frequently by Biggles, the fictional RAF fighter pilot in W.E.Johns' novels, is in some ways uncomfortably close to reality. The predominant practice of German pre- and proto-historic archaeology today is utterly methodical, but hardly inspirational (Härke 1991). The contrast to Anglophone archaeology could not be greater. In Germany, methods of source criticism, conventional chronology, documentation and fieldwork have been developed to a high standard, which compares favourably with much of contemporary practice in British and American archaeology. On the other hand, there has been in Germany very little reflection on the theoretical foundations and social context of the subject. The orthodox position has been spelt out by Fischer (1987:181): 'All theoretical methods of prehistoric archaeology…were developed in the 19th century' and 'One can justifiably state that the theoretical section of the methodology of our subject has been completed. Additions are to be expected in the practical section…' (Fischer 1987:194). Alternative positions exist within German archaeology, but carry little weight.

Fischer's attitude is reminiscent of the 'negativist' argument in physics which held that the work of physics was done save for the increasingly accurate measurement of the constants of nature (Barrow 1990:87). This was at the turn of the century, before relativity theory, quantum mechanics, nuclear fission, particle physics etc. Fischer's 'negativism' is not an isolated instance: there has been a peculiar reluctance in coming to terms with theoretical and practical developments of the discipline outside Germany after World War II. The basic textbook which has been used to introduce West German archaeology students to the methods of their discipline (Eggers 1959) was written over three decades ago. It contains less than five pages on scientific dating techniques, but 141 pages on conventional methods of dating (relative chronology, historical chronology, cross-dating, etc.). The lack of interest in theory (in the widest sense) is illustrated by a recent survey of the state of the discipline by an eminent West German archaeologist (Reichstein 1990): it is concerned exclusively with questions of organization, funding and training. In East Germany, a theoretical framework (that of historical materialism) had been imposed on the subject by

the political conditions there, but much of the theoretical 'debate' was artificial and sterile. Shortly after unification, Reinecke (1990:165) wrote about his East German colleagues: 'The vast majority of archaeologists…abstained from theory and dedicated themselves like busy bees to the assiduous collection and hoarding of data.'

What is it about the German tradition that has discouraged intellectual concerns in the discipline to a degree that is occasionally deplored even by staunch traditionalists (e.g., Kossack 1992:85)? How could German archaeology have remained in what is perceived by many outside observers as an empiricist and particularist stasis while achieving such technical excellence in fieldwork and documentation? Or does German archaeology, as its defenders see it, represent a stable tradition of artefact studies in a subject plagued by theoretical fashions?

This chapter does not pretend to give a comprehensive answer, but it will attempt to deal with three factors which have contributed substantially to shaping the German tradition of archaeology: intellectual, structural, and historical. These aspects are, of course, intertwined, and some overlap is unavoidable. In particular, it will be necessary to identify the historical strands of the intellectual tradition behind present-day German archaeology. In the following, the emphasis will be on *Vor- und Frühgeschichte* (Pre- and Proto-history), the subject dealing with the archaeology of the prehistoric and early medieval periods. Roman provincial archaeology, medieval archaeology and Near Eastern archaeology are separate academic subjects, although they are quite close to pre- and proto-history in their aims and approaches. Classical archaeology, by contrast, has adopted a very different outlook and has become the study of the art of Classical antiquity. The fact that the German shorthand term *Archäologie* is used mostly to refer to Classical Archaeology, but not to pre- and proto-history, has been a constant source of misunderstandings by non-German colleagues.

INTELLECTUAL TRADITION

While sweeping generalizations on the nature of scholarship in any one country are always dangerous, it is probably fair to say that German scholarship is pragmatic, and its emphasis is on application. This does not necessarily mean the application of scholarly research in the 'real world', but means also the application of basic rules in research rather than reflection on the rules themselves. The word *Theorie,* to German ears, sounds airy-fairy, it implies speculation 'without foundation' (i.e., without evidence), and it seems to exclude practicality which is considered highly desirable. To call somebody a 'theoretician', or to call something 'mere theory', invariably carries derogatory undertones. In order to avoid these connotations, German scholars outside the natural sciences avoid the term 'theory' as far as possible, and if they have to, they put it under the heading of 'methodology'. *'Methodik'* has a solid ring; it sounds practical, systematic, goal-oriented, efficient. A recent textbook on

research techniques in archaeological and related disciplines lists under *Methodik* not just strategy, planning and techniques of research, but also epistemology, philosophy of science, historical theory and history of research (Ziegert 1986: 105). Terminology and categorization demonstrate the primacy given to applied scholarship, an aspect which was recognized by Lord Acton as a key feature of German historiography as early as last century (Armstrong 1909:xiv). In keeping with this tradition, the emphasis of German archaeology is on 'craft' aspects of the discipline, not on intellectual aspects.

Another key factor of the German intellectual tradition is a desire for continuity and consensus. The young German scholar makes his reputation by following in the steps of his academic teacher, in marked contrast to the British system where the young scholar would attempt to make his reputation by demolishing his teachers. While some of the reasons for this continuity are sociological and structural (cf. below), others are quite clearly to be found in German notions of intellectual work and academic style, at least in the humanities. This shows, for example, in the hesitance to engage in controversial debate, in the rejection of polemic, in the distrust of intellectual fashions, and in the incomprehension of the Anglo-American 'publish or perish' phenomenon. One of the roots of this attitude may be the belief that paradigms in the humanities are not mutually exclusive, but cumulative (Fischer 1987:193)—a belief which might find some sympathy in Anglophone archaeology after decades of constant *Methodenstreit* (Sherratt 1992:139). The attention paid by German scholars to the history of research of every topic and every site illustrates this perception of continuity of work, and of the cumulative nature of knowledge. All these factors contributed substantially to the rejection by most German colleagues of the 'New Archaeology': its style was seen as abrasive, the works of its proponents tended to ignore publications older than two decades or written in languages other than English, and its spread appeared to spawn a multiplicity of short-lived fashions.

Third, and perhaps most important, German humanities have a strong positivist tradition. They strive towards the ideal of impartial and objective scholarship as formulated by the famous nineteenth-century historian Leopold von Ranke: 'wie es eigentlich gewesen' ('how it really happened'; von Ranke 1874:vii; cf. Carr 1990:8–9). This has been interpreted as an admonition to stick to the evidence and to 'solid facts', although this may be a narrow and selective interpretation of what Ranke actually meant. Armstrong has pointed out that Ranke had criticized Niebuhr's 'antiquarianism', and that he had discouraged research for its own sake, comparing it to the construction of cellar vaults without a house on top (Armstrong 1909:xiii). On the other hand, Ranke saw his own role as historian as being that of an observer writing descriptive narratives of political history (Vierhaus 1974). It is in this narrow sense of data-oriented, interpretation-free research that his influence is still widely felt in German historical and related disciplines, although the need for more theory has been increasingly recognized in German history after World War II (cf. Schieder & Graubig 1977).

The archaeological equivalent of Ranke's empiricist outlook is the antiquarian tradition which was founded by Gero von Merhart. He held the first German university chair of prehistory (at Marburg, from 1928), which accounts for the impact of his teaching and his approach on an entire generation of German archaeologists. It is interesting to note that the establishment of his chair was virtually contemporaneous with that of the Abercromby Chair at Edinburgh, the first British university chair of prehistory. At Marburg, the driving force behind the establishment of prehistoric archaeology as an academic subject was Paul Jacobsthal, the distinguished Classical archaeologist who emigrated to Oxford in the 1930s and wrote the classic volume on Celtic art (Jacobsthal 1944). While this clearly points in the direction of one decisive influence, another key element was Merhart's scientific training in geology (Kossack 1992:89–90). Scientific thought in Germany at the time was dominated by authorities like Max Planck who saw physical science as an essentially inductive enterprise, proceeding from measurements to ideas (Planck 1915:3), and who held that all anthropomorphic elements must be eliminated in order to achieve 'objectiveness' (Planck 1915:6–7; Planck 1950:13). This tenet, which is in stark contrast to Kantian idealism (Barrow 1990:90), appears to have influenced Merhart's approach to the study of the past. Induction in the scientific tradition, comparative typology derived from Classical archaeology, and source criticism derived from German historiography, became the main planks of Merhart's antiquarian school.

The majority of German archaeologists between the wars, and in the decades after World War II, were imbued with these ideas and approaches, having been taught by Merhart himself or by one of his former students. Other schools played a much smaller role, including those of the other two early professors of prehistory, Gustaf Kossinna and Max Ebert who had personal chairs from 1902 at Berlin and 1921 at Königsberg, respectively. As a result, a distinct tradition of prehistoric archaeology emerged, with a number of features which have remained characteristic to this day. The importance which the inductive method accords to the primary evidence has led to disciplined approaches to material culture, and to extensive description and documentation of the evidence, with close attention to detail.[1] Interpretation has taken a back seat because it is considered subjective and of temporary value only, while the 'facts' (i.e., documented evidence) are supposed to retain their value for a long time, in principle forever. A possibly related aspect is the disdain shown by many (but not all) German colleagues for the popularization of their subject: the ideals of scholarly detachment and perfection are hard to live up to when communicating research results to the general public. The comparative-typological method has resulted in an emphasis on artefactual evidence, on classification and on relative chronology. Typically, German publications of finds and sites have presented typology and dating under 'results', with little further interpretation. Source criticism, the careful discussion of factors of deposition, survival and recovery, is one of the most interesting methods of the German tradition of archaeology (Eggers 1951; Eggers 1959). It mirrors the concern for all aspects of source

criticism in the German school of history, and it represents an implicit use of the textual metaphor in archaeology decades before post-processualism.

The German archaeological tradition dominated much of European archaeology throughout the latter's culture-historical phase, until well after the middle of this century. There is still a good deal of research to be done on what exactly determined these influences. It would certainly be misleading to say that it was all due to economic interest and power, although this may have been a factor in central and eastern Europe. But intellectual traditions and historical ties seem to have played, at least, an equally important role (cf. Olivier 1991:259). One such connection existed between the Netherlands and northern Germany where similar approaches to settlement archaeology were developed shortly before and after World War II. The founder of modern Dutch archaeology between the wars, A.E.van Giffen, had a German motto which neatly encapsulates the essence of the German tradition: 'Die Interpretation schwankt, die Tatsachen bleiben' ('The interpretation changes, the facts remain'; Slofstra forthcoming). Very similar sentiments were expressed by Hallström who got archaeology off the ground in northern Sweden (Malmer 1993:114). British archaeology was within the orbit of the same tradition until the 1960s, at least as far as methods and approaches were concerned. On the other hand, Childe's work, and that of his students and followers, never displayed the reluctant attitude towards interpretation and generalization which characterized Merhart's antiquarian school. Still, the very marked differences between German and British archaeology today seem to have a comparatively short history, in spite of the specifically German characterization of archaeology as part of the humanities rather than the sciences (Smolla 1984:13). It would be misleading to extrapolate from the long-standing differences between the respective schools of history, or to assume that today's differences simply reflect opposing philosphical traditions (the Anglo-American analytical vs. the Continental phenomenological tradition).

Elements of the German archaeological tradition, its empiricist approach and methodical style of work were exported overseas by European scholars working in African, Asian and Latin American countries before and after their independence (cf. Kinahan 1995 and Tanudirjo 1995). German particularist and anti-evolutionary thought also had a major influence, through the ethnologist Franz Boas, on North American anthropology (cf. Mackie 1995) and (more indirectly) culture-historical archaeology in the early decades of this century (Trigger 1989:151–2, 186–7).

The influence of the German tradition in Europe declined after World War II, but this decline seems to have begun closer to the 1960s than to 1945. There are, of course, historical reasons for this development (and see Evans 1995). But it may also be a consequence of German archaeology failing to develop its own perspectives, and then failing to respond to the challenges of processual archaeology. Language, too, may be a contributory factor. Before World War II, German was the lingua franca of Continental Europe. Since the war, it has

gradually been replaced in this role by English in western Europe and Scandinavia, i.e., in exactly those regions where processual archaeology subsequently had some success since the 1970s. In central and eastern Europe, the German language, and with it the German intellectual tradition, retained their influence much longer. But since the disintegration of the Soviet empire, English has been making inroads, and it would not be surprising if archaeological thinking were to swing with it. A case of scholarship following language following politics?

STRUCTURAL CONTEXT

The image of Germany as a modern and efficient society is probably true for much of the state archaeological services and most museums, but hardly for the academic sector. The present university system was created in the early nineteenth century, as part of the restructuring of the Prussian state in the wake of the defeat by Napoleon. Although there was some tinkering around the edges of the system in the 1970s, its core has remained essentially the same. Central to the system is the independence of all professors, the much-quoted 'freedom of research and teaching' which is enshrined in German law. The creators of this system held that professors should teach the results of their current researches. This nineteenth-century ideal was overtaken by reality a good while ago, but the emphasis of the unreformed system remains entirely on research. This has resulted in unclear and disjointed course structures, and more often than not in unsatisfactory standards of teaching.[2] But the unregulated growth of the university system, facilitated by the absence of admission controls for most subjects (including pre- and proto-history), has not made it any easier to maintain academic and teaching standards.

The strong position of the professors has had profound consequences for course contents, career structures, and the nature of academic discourse. Only professors are entitled to give formal lectures, to supervise dissertations and theses, and to conduct examinations. Given the comparatively small number of professors and the absence of a regular provision for external examiners (from outside the university concerned), a professor will routinely examine every thesis previously supervised by him. The situation is worse in small subjects like archaeology where there are often only one or two professors at each university department, and where the doctorate is still considered the normal conclusion of university studies for the majority of students. Also, the professor is the obvious person to turn to for references, so that not just the success of one's studies but also one's job prospects depend on the same, one person. For somebody staying in the university system to become an academic teacher, the dependence continues until he has become a professor himself. In order to qualify for a professorship, he has to obtain a first degree, a doctorate (Dr. phil.), and a habilitation (a second doctorate)—a long apprenticeship reminiscent of a guild

system, which is entirely in keeping with the 'craft' element of the intellectual tradition (cf. above).

The structures of dependence create small, tightly knit schools around individual professors. They also force young scholars to strike a balance between originality (which might alarm and upset older professors) and conformity (which is not a sufficient condition for elevation to a professorship): to be a successful academic in the German system requires discipline, flexibility and a clear perception of the limits of one's possibilities (Plessner 1956:31–3). These are the sociological factors behind the continuity of work identified above as a distinctive feature of the German tradition. Discourse in such an environment has carefully circumscribed limits, and discussion mostly concentrates on aspects of the evidence (Sommer 1991). If there is debate at all, it tends to turn into tribal warfare, in contrast to Anglophone archaeology where debates have recently hinged on theoretical positions and therefore resembled religious wars instead.

There are two other structural factors which act as constraints. One is the employment conditions: the vast majority of German archaeologists, in universities, state archaeological services and most museums, are civil servants. They are thus subject to the code of a hierarchical organization whose ethics and structures militate against individual initiative and open debate. The other factor is the academic publishing system which depends heavily on subsidies from research funds and state institutions, leaving less room for ideas and imagination than fully independent publishers would normally have. On the other hand, it can provide for the publication of catalogues and site reports which would find no taker in commercial publishing. It is also clogged up with the numerous doctoral theses which German university regulations require must be published in full.

The above is a sketch of the pre-war German, and the post-war West German, system. Different constraints operated in East Germany from 1945 to 1990 (Behrens 1984). Aspiring archaeologists were trained at only two universities, and admission was strictly regulated in order to produce carefully planned numbers of graduates for existing job vacancies all of which were, of course, in the state sector. A small number of select graduates and junior colleagues could obtain positions in the Academy of Sciences at Berlin where they had preferential access to funds, publication opportunities and travel abroad. The privileges of this academic elite caused a good deal of resentment among colleagues outside the Academy, but they were obtained at a price: political conformism and, in most cases, Communist Party membership. Outside the Academy, promotion to senior posts was also conditional upon Party membership, at least in the majority of cases of directorships and professorships after the post-war transition period. For ordinary archaeologists, opportunities for research, publication and contacts abroad depended entirely on the permission by superiors. These repressive conditions did not allow for a lively intellectual atmosphere, and few colleagues managed to resist the pressure to conform (Coblenz 1992). Since unification, most of East German archaeology has been restructured along West German lines, leading to the dissolution of the Academy and its attendant two-tier system

within archaeology, but creating new pressures and constraints in the process (Gringmuth-Dallmer 1993; cf. below).

HISTORICAL BACKGROUND

The vicissitudes of German history in the nineteenth and twentieth centuries have affected archaeology as much as they affected society at large. They have left German archaeologists ill at ease with the history of their own discipline, preferring to present *Forschungsgeschichte* as lists of important scholars and excavations instead of focusing on the history of ideas, their development, use, and misuse. This is not the place to discuss in detail the social and intellectual history of German archaeology (cf. Härke forthcoming). The discussion here has to be limited to an outline of some key factors which influenced the German tradition.

A number of historical strands and intellectual movements have contributed to German archaeological thought over the last two to three centuries (Wahle 1964: 3–131; Fischer 1987; Kernd'l 1991:19–35; Kossack 1992). Probably the oldest of these is the concern with Classical antiquity *(Altertumskunde)*. It created ancient history and philology as well as Classical archaeology and Roman provincial archaeology, and it contributed to the German tradition the art-historical paradigm (formulated by Winckelmann 1764) and essential elements of the antiquarian approach (cf. above). While this strand was institutionalized early in the academic context of Classics, pre- and proto-historic archaeology struggled for recognition throughout most of the nineteenth century, drawing on Romanticism and anthropology.

The Romantic movement of the eighteenth century led, among other things, to an interest in peoples, their differences and their respective histories. This interest was given a scholarly expression by Johann Gottfried Herder and others. It strove for comprehension of roots and differences, and saw its intellectual home in the humanities where it gave rise to ethnology, folk studies (*Volkskunde,* essentially European ethnography), Germanic philology and German history. By the early twentieth century, its archaeological offshoot had created an ethnic paradigm as the backbone of a 'national archaeology', had developed its own approach to the evidence ('settlement archaeology', in effect the ethnic interpretation of artefact distributions; Veit 1989), and was represented in the university system (Gustaf Kossinna's personal chair of Germanic prehistory at Berlin University, from 1902). Part of this rise was due to the political background of the nineteenth century. The aristocratic elite of the many German states of the eighteenth century had scant regard for the German past, and kings like Frederick the Great of Prussia preferred to look for inspiration to Roman antiquity (Kernd'l 1991:21). The defeat of the old aristocracies, the Napoleonic wars and their aftermath led to a search for national identity and to a struggle for German cultural and political unity. The achievement of the latter in 1871 created new tensions in Europe, fuelling nationalist sentiments. By the end of the

nineteenth century, the rising bourgeoisie had been joined in its quest for the national past by Kaiser Wilhelm II who declared that 'we should educate young Germans, not young Greeks and Romans' (quoted by Smolla 1984:12).

The last strand to emerge in the intellectual history of German archaeology, and also the shortest-lived, was the anthropological perspective. Its origins lay in rationalism and the natural sciences, and its aim was explanation. Its contribution to nineteenth century prehistoric archaeology was the evolutionary paradigm, and the establishment of close links to physical anthropology. The driving force behind anthropological archaeology was the famous Berlin surgeon Rudolf Virchow (Vasold 1988) who vigorously opposed Romanticism and simplistic diffusionism (Ottaway 1973). After his death in 1902, the anthropological approach went into steep decline before it could put its cosmopolitan and scientific stamp on German prehistory. It is, perhaps, symbolic that the same year which saw Virchow's death, also saw the (admittedly late and grudging) grant of a personal chair to Kossinna, the foremost champion of 'national archaeology'.

Kossinna has often been portrayed as the evil mind behind all chauvinist and fascist exploitation of archaeology. But it was a scholar from the (then) Soviet Union, Leo Klejn, who first pointed out that Kossinna must be seen in the context of his time (Klejn 1974). Judged from this perspective, Kossinna is no longer such an exceptional figure, but fits (albeit as an extreme example) into the political context and intellectual development of the discipline in Europe. His aims (if not his German perspective) were shared by French archaeologists exploring the Celtic roots of their nation, and by Polish archaeologists attempting to demonstrate the Slav ancestry of prehistoric cultures on their territory. The historical context of 'national archaeology' is also demonstrated by the identity-enhancing role it has played in frontline states (the Masada excavations in Israel being a poignant example), and by its emergence in post-colonial situations (cf. Tanudirjo, 1995). Finally, Kossinna's methodology had been anticipated by Scandinavian scholars (Oscar Montelius' retrospective method), and was shared by British colleagues (V.Gordon Childe's culture concept) although the interpretation of cultures differed somewhat (Sherratt 1989; Veit 1984). It would even be possible to argue that Kossinna's thinking included progressive aspects: he had stressed the dual nature of prehistoric archaeology, being a historical as well as a scientific discipline (Smolla 1984:13).

Kossinna's negative image has been exacerbated by the misuse of his methodology after his death in the Third Reich when it was elevated to the status of dogma and exploited for the legitimation of racist and expansionist policies (Arnold 1990; Arnold & Hassmann forthcoming; McCann 1987; McCann 1990). But the Nazi exploitation was facilitated by the state of German archaeology (Kossack 1992:94): the positivist orientation did not offer a theoretical critique of historicizing, ethnic interpretations of prehistoric evidence, and the demise of the anthropological paradigm had removed an intellectual alternative. Also, the massive support for, and expansion of, prehistoric archaeology during the Third

Reich, coupled with the conviction that archaeology, too, could contribute to the creation of a 'new Germany', must have proved seductive to many German prehistorians who collaborated, or at least acquiesced, on a large scale (*pace* Kossack 1992:94–6). On the other hand, it should not be forgotten that there were archaeologists who refused to go along with the Nazi transformation and exploitation of archaeology. And it must be stressed that the majority of those who collaborated with the Nazis, be it out of conviction or opportunism, still produced good work in the sense of the German 'craft' tradition. There are contemporaneous examples from other disciplines (the most prominent being the philosopher Heidegger) which demonstrate that good or even outstanding scholars can fall prey to a seductive ideology, or succumb to a repressive system.

This rule was demonstrated again, although in a different way, in post-war East Germany where archaeology, generously funded by the state, was exploited within a theoretical framework which had become dogma. Again, most archaeologists collaborated with, or adapted to, the political system, although there were exceptions, too (Coblenz 1992). And yet again, most archaeology achieved high technical standards, irrespective of the lip-service paid to Communist ideology. One may argue that East German archaeology missed the opportunity offered by the re-introduction of the evolutionary paradigm within historical materialism to develop a credible counter-position to the anti-theoretical stance of their West German colleagues (cf. below). But that was, perhaps, inevitable. The limits imposed on the intellectual debate by the political regime rendered most of it sterile, and made it easy to dismiss East German approaches in the historical and social sciences as 'vulgar Marxism'. The fact that only the most reliable conformists were allowed to travel, and the lack of hard currency to buy western publications, increased the isolation of East German archaeologists from the outside world and its ideas, and made it difficult for them to contribute to its debates.

In the meantime, their western colleagues had gone down a different road. As a consequence of the political exploitation of prehistory by the Nazi and Communist regimes, post-war West German archaeology suffered from the 'Kossinna syndrome' (Smolla 1980), the fear of over-interpretation. This led to a retreat into description, and the rejection of almost any form of interpretation which went beyond typology and chronology (Narr 1966:382). The structures of German archaeology (cf. above) meant that this retreat was not a temporary shock reaction, but became enshrined in a narrow definition of 'serious' scholarship. In a sense, the rejection of ideology had itself become an ideology. The irony of the situation does not end there because the retreat was, of course, to an atheoretical, empiricist and positivist position of the kind which had facilitated the Nazi exploitation of archaeology in the first place, and which may still have undesirable political consequences in spite of its claim to 'objectivity' (cf. Kinahan, Ch. 4, this volume). This point has not been widely realized in Germany because the Nazi past was never openly discussed in the discipline, neither in the East nor in the West. Any references in the archaeological

literature to the Third Reich have been limited almost entirely to discussions of the shortcomings of Kossinna's methods. The critical investigation of the links between prehistoric archaeology and Nazi politics has been left to scholars from outside the discipline (Bollmus 1970; Kater 1974). This has been defended, with some justification, on the grounds that no scholar can write a dispassionate, contemporary history or sociology of his own discipline. While this attitude is understandable, and even to be expected given the German ideal of detached scholarship, it has deprived German archaeology of the opportunity to reflect on a number of critical issues.

PERSPECTIVES

This opportunity has now been created by a new and unexpected factor: the collapse of the East German regime in 1989, and unification in the following year. However, it was not the meeting of West and East German minds which produced something new, but the fulfilled dream turning sour within a couple of years. In the academic world, as elsewhere, it has become a one-sided process, and archaeology at universities, in museums and in the state services has undergone traumatic changes (Gringmuth-Dallmer 1993). The 'open and balanced debate between eastern and western archaeologists' that the East German Reinecke called for in the year of unification (Reinecke 1990:166), has not materialised. Instead, western structures of organization have been extended to the East, particularly in universities and museums, and all eastern colleagues have been subjected to 'evaluation', a mixture of academic appraisal and political screening. The results have, predictably, been large-scale dismissals and job losses, as well as alienation and charges of *Siegerjustiz* (justice meted out by the victors). This has been countered with the argument that some 'cleaning-up' was necessary after forty years of repression and collaboration.

From the mess of the political and organizational process, two trends have emerged which may have a bearing on archaeology. The first is the retreat of the humanities in eastern Germany into traditionalism,[3] and this quite clearly includes archaeology. If this trend continues, there are unlikely to be any changes in the outlook and philosophy of German archaeology as a whole. The other trend is pointing in the opposite direction. The consequences of evaluation have left a large number of eastern colleagues disaffected and angry enough to speak out. They have also made a sufficient number of western observers uncomfortable and critical enough to facilitate a public discussion about the entire process, and about the interrelationship between archaeology and politics in general (Härke & Wolfram 1993). This debate may yet have a considerable impact on the attitudes of German archaeologists, East and West. It could also bring German archaeology into the European post-processual debate. Change in the discipline may also be facilitated by the considerable number of younger West German archaeologists who in the wake of the re-organization find themselves propelled into senior posts in the East. On the other hand, such a

rejuvenation has happened before (during the expansion of the West German universities in the 1970s), without any noticeable impact on the outlook and direction of German archaeology.

At this early stage, it is hard to tell which of the trends will eventually prevail, and where it will leave the German tradition of pre- and proto-history. It has recently been suggested that the theoretical foundations of a post-positivist archaeology are already in existence (Sommer 1991:213). But these elements (source criticism, critique of ideology, and search for middle-range theory) have been around since at least the 1960s. Given the background and history of the discipline, it is likely that any theoretical and methodological re-assessment will keep pre- and proto-history firmly within the orbit of the historical disciplines, rather than leading to an alignment with cultural anthropology and the sciences (as happened with the 'New Archaeology') or with social anthropology (as appears to be happening now in the Netherlands). Some change may be brought about by the continued drying-up of state funds for archaeology, forcing German archaeologists to find alternative funds and to re-assess their attitude towards popularization of their subject. In the universities, the catastrophic over-crowding may force the authorities to carry out a fundamental reform of the system, but it is as yet difficult to see what shape this reform will take, and what its consequences will be for the academic training of German archaeologists. Whatever happens, intellectual tradition and structural constraints will ensure that any change will be gradual and evolutionary.

In the German tradition of scholarship, there is no place for revolutions, and there are no short cuts to knowledge. Goethe (no mean archaeologist himself; see Todd 1985) has mapped out, in his drama *Faust,* what happens to a scholar who eschews the slow and methodical approach and opts for the short cut: after a bargain with the devil to provide the answer as to 'what keeps the world together', Doktor Faust went to hell. If the German view of scholarly virtues has any eschatological implications at all, Faust will be joined there in due course by a good number of British and American archaeologists.

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NOTES

1 These are virtues which outside observers readily associate with German scholarship in general. An obituary of the egyptologist Bernard Bothmer (New York University) observed: 'he remained a German in his bearing, and in his general way of life, especially in his discipline in matters scholarly…He abhorred casual behaviour, lack of commitment, charlatanism and illiberal attitudes —

intolerance should be reserved for matters of real importance like an incorrectly taken photograph' (*The Times,* 3 December 1993, 23).

2 This problem is widely recognized in Germany. Professor Grottian of the Freie Universität Berlin recently suggested that one-fifth of German university professors ought to be sacked because of poor teaching performance (report in *Times Higher Education Supplement,* 4 December 1992).

3 The *Rückzug ins Traditionelle* has been identified by the historian Kurt Nowak from the University of Leipzig, Germany (in *Wirtschaft und Wissenschaft* 3/93, 5).

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CHAPTER THREE
THEORETICAL TRENDS IN INDONESIAN ARCHAEOLOGY

DAUD A.TANUDIRJO

INTRODUCTION

The first human occupation in Indonesia can be dated back as early as 2 million years ago, as proven by the discoveries of human fossils in Java. During the prehistoric period racially different groups of people inhabited the archipelago. Some of these are still traceable in the present population. Indonesia entered the historical stage in the fourth century AD when stone inscriptions written in Indian characters started to be issued by Mulawarman, the ruler of Kutai (East Borneo). Since then, several kingdoms which were influenced by the Hindu culture of India emerged in Sumatra, Java and Bali. The Hindu cultural influence, however, was limited to those islands. In the remaining areas, people maintained their traditional ways of life.

In the thirteenth century, the first Islamic kingdom was founded in the northern part of Sumatra and Islam became the emerging cultural and political force. Following the collapse of the last Hindu kingdom in Java in the fifteenth century, the dominance of Hindu culture declined and was replaced by that of Islam. The influence of Islam became even stronger when an Islamic kingdom came into being in Java. The extensive trading network under the Islamic rulers was used effectively as the main medium of Islamization in Indonesia. Due to this process the majority of the population is now Moslem.

Towards the end of the sixteenth century Europeans came to the archipelago. Initially arriving as merchants, the Dutch soon assumed control of the local rulers and colonized Indonesia. Although for more than 350 years Indonesia was for all intents and purposes under the Netherlands, European culture had relatively little impact on the indigenous people. In 1945, Indonesia declared its freedom from any colonial government, and since then it has been an independent country.

Given such a diverse environment and the complex history of the Indonesian people, one cannot speak of a uniform culture for the entire archipelago. Successive imported cultures, i.e., Indian, Islamic and European, have never totally replaced the original one. It seems there are always opportunities for people to maintain their own traditional way of life rather than to adopt a new one. This

is evident in the occurrence of enclaves of people living according to their traditional ways. Even today when Indonesia is becoming an industrialized country there are still several traditional groups living in the remote parts of the country.

At present, the total population of Indonesia is slightly over 180 million. Nearly 90 per cent of them live on the island of Java, the heart of the country. There are over 300 ethnic groups settled in the archipelago and more than 250 languages are spoken. Although the majority of the population are of southern Mongoloid stock and speak Austronesian languages, there is still a considerable cultural variety within it.

This does not, however, mean that one should always view Indonesia as a scattered collection of cultural groups. On the contrary, it should be seen as a mosaic-like picture with a single cultural image. As stated in its motto, *Bhinneka Tunggal Ika* (Diversity in Unity), Indonesia is a country of diverse cultures but essentially one. This should be kept in mind when we approach Indonesia as presented in this chapter.

Indonesian archaeology is generally divided into four phases which are considered as applicable to the entire country: prehistoric archaeology (concerning materials from c. 2 million years ago—fourth century AD), Classical archaeology (fourth century AD—c. fifteenth century AD), Islamic archaeology (c. fifteenth century AD-c. eighteenth century AD) and epigraphical archaeology (concerning the study of inscriptions of the Classical and Islamic periods).

THE DAWN OF INDONESIAN ARCHAEOLOGY

The history of archaeology in Indonesia began in the early eighteenth century when some Europeans started to pay attention to the ancient artefacts and monuments found there. They were in fact individuals of various interests, mostly naturalists and government officials, who were curious about mysterious ruins, exotic artefacts and local history.

Rumphius, a botanist and zoologist, is held to be the first scholar who recorded prehistoric artefacts. He devoted two chapters of his book *D'Amboinsche Rariteitkamer* (published in 1705) to descriptions of stone adzes, bronze celts and a kettledrum, complete with the legendary stories behind those objects (Heine-Geldern 1945:129). In 1730, Barchewitz reported a kettledrum found on the small island of Luang, east Indonesia (Soejono 1969:71). The existence of ruins of Hindu temples at the village of Prambanan (central Java) was first recorded in 1733 by Lons, a Dutch official who first visited the interior of Java, and then by another European, von Boekholtz. These records led Engelhard and Cornelius to carry out more intensive explorations of the ruins (Soekmono 1969: 93).

The founding in 1778 of the Royal Batavian Society for Arts and Sciences has often been seen as the echo of the Enlightenment spirit of Europe (Wibowo 1976:

64; see also Trigger 1989:57). As many of its members were art and antique collectors, the institution soon had at its disposal a museum with a great collection of prehistoric, ethnographic and exotic artefacts. Later on the institution came to be known as the Batavian Museum and is now called the National Museum.

The works of Sir Thomas Stamford Raffles, a Governor General during the British interregnum in Java (1811–16), are important. Many of his assistants throughout Java were sent to collect historical data on ancient monuments and he availed himself of the chance to visit many of them. One of the notable results was the rediscovery, in 1814, of Borobudur, the largest Buddhist monument in the world. Another important work was the publication of his two-volume book *The History of Java,* in which he described many of the antiquities of Java (Soekmono 1969:94). Raffles also paid considerable attention to Old Javanese and Sanskrit inscriptions, and two chapters of his book contain a description and simple classification of them. Although he was unable to utilize the inscriptions adequately, his work can be considered as the beginning of historical archaeology in Indonesia (Wibowo 1976:65).

From a theoretical point of view, little can be said about this stage of Indonesian archaeological enquiry. There was no indication of any theoretical framework applied to explain the existence of the antiquities. If there was explanation of certain antiquities, it was too speculative to be scientific. Although Raffles attempted to be an objective historian, his history of Java is much coloured with legendary stories and interpretations told him by local people (Wibowo 1976:65). It seems that this situation was much the same as the early antiquarian stage of Europe (Trigger 1989:45–9; Sharer & Ashmore 1979: 35). Significant though it might be, its contribution to Indonesian archaeology lies merely in the fact that it brought to the awareness of the public both the richness of Indonesian antiquities and the importance of such antiquities for local historical reconstruction.

THE AGE OF EXPLORATION

Raffles' exhaustive effort to shed light on the history of Java appeared impressive to the Dutch administrators. In 1822, soon after the British withdrawal, the re-established Dutch government set up a Commission for the Exploration and Conservation of Antiquities. It was the first official institution founded to accommodate the Europeans' interest in antiquity. It did not work because most of the Europeans interested in the antiquity of Indonesia at this stage were amateurs such as administrators, artists and missionaries, who had a personal interest in archaeology, or at most, were scholars of natural science who, of course, did not want to become involved formally with an official/government institution such as the Commission. In addition, some of them had already joined the Batavian Society of Arts and Sciences where they could work independently. As most people interested in antiquity preferred to work outside the

Commission, almost nothing could be done by the Commission to carry out its programme.

In the event it was due to such independent work that, by the middle of the nineteenth century, many ancient ruins of Java were recovered and rescued (Soekmono 1969:94).

Exploration for museum collections began in the 1850s. Leeman at the Museum of Antiquities, Leiden, began the systematic study of a stone adze collection sent from Indonesia. While in the Batavian Museum, van Limburg Brower and Pleyte conducted similar studies. This work was continued with publication of a catalogue listing archaeological artefacts stored in the Batavian Museum by Groeneveldt in 1887 (Soekmono 1969:94).

Islamic antiquities attracted more attention as exploration was extended. In 1884, a report on the occurrence of Islamic ancient tombs came into the Batavian Museum. Since then, Islamic antiquities have been considered significant sources of art and historical studies (Tjandrasasmita 1976:107–9). Another kind of historical source which attracted many linguists at that time were inscriptions of the Hindu kingdoms. Friedrich, Kern, Stuart and Holle were among the first linguists who tried to translate and interpret those inscriptions (Wibowo 1976:66–7).

In 1885, an archaeological society was established by the Dutch in Yogyakarta. This institution aimed to encourage and coordinate more scientific exploration of ancient monuments in Java, undertaken especially by Europeans. Ijzerman's thorough investigation of Borobudur resulted in the recovery of the hidden reliefs formerly covered in the base of the monument. Excavations were carried out in the temple ruins of Prambanan by Ijzerman and on the Dieng Plateau by van Kinsbergen (Soejono 1969:71; Soekmono 1969:94–5).

In 1888, the first fossil of ancient *Homo sapiens* in Indonesia was dug up from a marble quarry near Tulungagung by van Reischoten. Two years later, after a massive excavation in the small village of Trinil (East Java), *Pithecanthropus erectus* fossils were found (Heine-Geldern 1945:129; and see below).

Steinmetz investigated megalithic monuments in Besuki (East Java) in 1898 and in the following year the existence of ancient terraces and menhirs on a mountain nearby were reported by Kohlbrugge (Soejono 1969:72).

The enormous amount of data gathered from large-scale exploration of ancient monuments has revealed beyond any doubt characteristics of Indian art and architecture. So far, the interpretations of the oldest inscriptions have produced the same conclusion. This led scholars to start to question the origin of the monument builders. Linguists, such as Kern and Holle, were inclined to speculate on the possible colonization of Indonesia by Indian people. Such a notion provided the basis for the future development of Indian migration theory in Indonesian archaeology.

Another important phenomenon that appeared at this stage was the eagerness to undertake systematic classification of certain kinds of prehistoric artefacts, as was done by Leeman, van Limburg Brower and Pleyte on stone adzes, and by

Meyer and Foy on bronze kettledrums. Such efforts are of course significant for archaeological theory-building, as the classification itself is in fact a low-level theory (Trigger 1989:20–1). This is also evident in the work of Pleyte. His classification of stone adzes and their geographical distribution was fundamental to the rise of prehistoric migration theory in Indonesia (Heine-Geldern 1945: 129). Meyer and Foy are admired as pioneers in the comparative study and classification of kettledrums in Southeast Asia. Apparently they followed the comparative method disseminated by Worsaae in Europe (Malina & Vasicek 1990:38) and even adopted his proposition to the effect that the use of bronze was diffused from the Southeast Asian mainland to Indonesia. They suggested that kettledrums were originally produced in Cambodia and then distributed widely to the whole of Southeast Asia, including Indonesia (Soejono 1969:71).

Palaeoanthropological research carried out by Dubois had promoted the human evolution theory in Indonesia. He came to Indonesia in search of the 'missing link', since Darwin stated in *The Descent of Man* that such an ancestor must have lived in the tropics. Driven by his obsession, he undertook excavations in several sites in Sumatra and Java and found fossils which he considered to represent ape-men which he named *Pithecanthropus erectus* after Haeckel's hypothetical name for the link between humans and apes. Although his proposition is now regarded as incorrect, it forced many scholars at that time to acknowledge the existence of transitional hominids in human evolution (Theunissen, de Vos, Sondaar & Aziz 1991:40–3) and also attracted many palaeoanthropologists to do research in Indonesia. Unfortunately, Dubois focused his research exclusively on human remains: he seemed to disregard artefacts that might have been used by the ancient hominids. Archaeologically, therefore, his work had little value.

From the foregoing it is clear that at this stage Indonesian archaeology was characterized by more coordinated explorative work. More varied antiquities were collected and studied by scholars with proper expertise so that the results were relatively scientific. Classification was seen as a means of extracting information from the data available. Towards the end of this stage, comparative studies were undertaken by some scholars which, in their turn, gave way to the initial use of theories borrowed from Europe. Migration, diffusion and human evolution theories were introduced to provide possible explanations. This stage, therefore, witnessed the laying of a cornerstone for the development of theories in Indonesian archaeology.

THE AGE OF SYNTHESIS (1900–45)

The beauty of Indonesian antiquities has been internationally recognized ever since they were displayed at the International Colonial Exhibition held in Paris in 1900. It made the Dutch government set up a Commission in the Dutch Indies for Archaeological Research in Java and Madura in 1901. Headed by Brandes, the Commission worked efficiently. Unfortunately, after Brandes died in 1905, it

practically collapsed. In 1910 Krom was appointed to lead the Commission. As he was aware of the restrictions preventing it from carrying out continuous research and preservation, he reorganized the Commission as a permanent institution, which in 1913 was inaugurated by the Dutch government as the Archaeological Service in the Dutch Indies, whose task was to coordinate research and the preservation of archaeological remains. From that time on, archaeological activities were pursued within the framework of this governmental organization. After Krom returned to the Netherlands, in 1915, the Archaeological Service was presided over by Bosch and Stutterheim respectively, until World War II broke out in 1942 (Soekmono 1969:95).

Under the Commission and the Archaeological Service, systematic research increased considerably. In the early stage, the focus was mainly on historical archaeology, i.e., epigraphy, Classical and Islamic archaeology, since the scholars involved in those institutions were linguists (philologists) and historians. Only after 1923 was prehistoric research included in the Archaeological Service's activities. Towards the end of this stage, a number of native Indonesians interested in archaeology were trained. Some of them were sent to work as assistants for the branches of the Archaeological Service in Bali and central Java established in 1938 (Soekmono 1969:96).

Classic monuments and artefacts were the main objects of study at this initial stage. Many of them were recorded in detail and excavated systematically, so that monographs on them, e.g., Candi Jago and Singosari, could be published. Massive restoration work was carried out in Borobudur and Prambanan. It seems probable that such restorations were primarily aimed at preservation or were undertaken for scientific purposes, whereas tourism was probably considered as the second or third priority. This was implied by the debate between Dutch archaeologists concerning how to restore or rebuild ancient monuments. Some scholars suggested that restoration should be done on paper only. Other scholars, however, insisted on restoring ancient monuments at the sites as well. Although they had different opinions, they agreed that restoration of ancient monuments should be directed to scientific ends. In general, research into classical remains was directed towards the study of art and architecture. Comparative studies on architectural style, spatial arrangement within monuments and ornamentation were among the most popular research topics.

At the same time, epigraphical research was eagerly pursued. Brandes, Krom and Bosch, who were basically philologists, were interested primarily in inscriptions which provided them with the information they needed for writing the history of Indonesia. Both Classical and Islamic inscriptions were collected, translated and studied comparatively. It was in this context that in the 1920s a few Indonesian scholars began to take part in archaeological research. Hoesein Djajadiningrat and Poerbatjaraka were the first notable linguists to undertake such research (Tjandrasasmita 1976:109; Wibowo 1976:78). Epigraphical studies were also aimed at acquiring data relating to archaeological remains. In

such circumstances, it was not surprising that historical archaeology increased rapidly.

Although prehistoric research had not been included in the work of the Archaeological Service by 1923, this did not mean that prehistoric research ceased to exist. In 1902–3 Fritz and Paul Sarasin (originally from Switzerland) carried out the first systematic excavation in a prehistoric cave in south Sulawesi. Kruyt and Killian thoroughly surveyed megalithic monuments in central Sulawesi. Starting in 1902, Heger reclassified bronze kettledrums. An extensive palaeoanthropological excavation was undertaken by Selenka in Trinil in 1907–8 (Soejono 1969:73).

After it came under the auspices of the Archaeological Service, prehistoric research was considerably expanded. Systematic excavations were carried out at various sites (caves, kitchen middens, megalithic monuments, settlements, urn burials) throughout almost the entire archipelago. Palaeolithic artefacts were first discovered in Sumatra and Java. Since 1934 palaeoanthropological research has brought to light various fossil hominids (Soejono 1969:74–86).

At this stage, scholars engaged in archaeological research were not satisfied only to present data. They did not stop at formal analysis, but went on to spatial and temporal analyses. They were urged to pursue further interpretations. In doing so, they tried to implement new methods and developed theories which had intially been introduced in the explorative stage. Indonesian archaeology then entered its descriptive and synthetic stage, in which tentative explanations were postulated.

Such studies were apparent in the works of scholars studying Hindu, Classical and Islamic archaeology. They tried to place the archaeological materials within a broader context by comparing them with those of the Southeast Asian mainland and India. Historic archaeology which was prevalent at this stage provided scholars with a temporal dimension for their data. Krom, for example, had realized fully that epigraphical studies could provide a framework for artefacts and monuments to be put into chronological order (Wibowo 1976:73). These methods led Indonesian archaeology to adopt a culture-historical approach.

Most scholars seemed to agree that the existence of Hindu and Islamic cultures in Indonesia was the result of several migrations from South Asia as previously proposed at the end of the explorative stage. Nevertheless, they put forward three different propositions as to how Hindu migrations to Indonesia had taken place: first, as suggested by Berg and Mookerji among others, that Hindu culture was brought by Indian noblemen and warriors *(ksatrya),* who at that time came to colonize Indonesia. However, that proposition was severely criticized by Krom and Bosch. According to Krom, Hindu culture had been introduced by Indian traders *(waisya),* who then lived and intermarried with the local population and persuaded local rulers to adopt their culture (Bosch 1961). In the 1930s another new perspective was offered by van Leur (1955), who made a thorough study of the economic history of the archipelago. He contended that indigenous initiative had played the greatest part in the adoption of Hindu

culture, since Indonesians had also arrived, stayed and even studied religions in the Asian subcontinent. When they returned home they brought Hindu culture with them. This proposition agreed with that of Bosch. Although Bosch partly accepted the role of a small group of 'clerks' in importing the Hindu culture, he was convinced that Indonesian students, who at that time studied religion in India, played the greatest role in disseminating Hindu culture (Bosch 1961:11–12). Since then, most scholars (see Coedes 1968; Mabbett 1977) have agreed on the vital role of Indonesians as transmitters of Hindu culture to their own country. A similar view seems to be adopted in explaining the appearance of Islam. The Islamic culture of Indonesia has been perceived as the result of cultural interaction between foreigners and indigenous people.

In the case of prehistoric archaeology, by the 1970s, classification of artefacts was still aimed only at summarizing data. Later, however, such classification became the main tool of artefact analysis, into which the temporal and spatial dimensions of artefacts were incorporated. Stone and bone artefacts were considered older than bronze and iron artefacts. After the discovery of artefacts considered to be palaeolithic in 1924, the prehistory of Indonesia was divided into the Palaeolithic, Mesolithic, Neolithic, and Bronze and Iron Ages (Heine-Geldern 1945; Soejono 1969:74). This was obviously influenced by Thomsen's Three Age system, which was elaborated by Worsaae and Lubbock (Eddy 1991: 29). Although it was not fully applicable, this relative chronological framework underpinned the thought of prehistorians working in Indonesia.

Artefact classification was evidently fundamental to theory-building in Indonesian prehistory. Referring to Pleyte's stone adze classification, both van Stein Callenfels and Heine-Geldern suggested the geographical and chronological distributions of stone adzes in the archipelago. On the basis of their findings, they postulated that several waves of migrations had taken place in the neolithic period (Heine-Geldern 1945:129–42). Using a similar method, scholars who studied bronze kettledrums and megalithic structures, provided additional support for the previous supposition that such cultures were brought into Indonesia by migration or diffusion. Perry (1923) hypothesized that the Megalithic of Indonesia was introduced by ancient Egyptians. This hypothesis was intended to support his hyperdiffusionism (Trigger 1989:153).

The most influential prehistoric migration theory was set forth by Heine-Geldern. As an art historian of the Vienna School, he based his theory primarily on the results of his comparative art studies of prehistoric artefacts. Heine-Geldern was an Austrian who tried to establish the spatial and temporal distribution of prehistoric artefacts based on comparative art studies or so-called 'art archaeology'. Based on the results of his studies, he suggested the possibility that waves of migrations from Southeast Asia into the Indonesian archipelago and Pacific region had already taken place.

To some extent, his opinions and approach were not so different from those of Dutch scholars at that time, such as van Stein Callenfels and van der Hoop. However he was admired for his ability to put together or synthesize theories of

the distribution of people in his study area (including linguistic theories), so he proposed his own new theory which was supported by convincing data. His migration theory had a strong influence on Dutch scholars and even today is still cited by many Indonesian archaeologists. He had, however, himself been influenced by Schmidt's theory (Trigger 1989:152) on the development of Asian languages (Heine-Geldern 1945:138). In building his theory, therefore, he brought together the results of this research in art and archaeology and linguistic theory. Finally he concluded that there had been two major migrations from the Southeast Asian mainland into Indonesia. First, between 2,500 and 1,500 BC, there came people speaking Austronesian languages who brought with them neolithic culture. About 500 BC, the second migration took place bringing metal technology and a complex megalithic culture. The latter was attributed to the Dongson Culture, which had been influenced by the Hallstatt and the Caucasian Iron cultures of Europe (Heine-Geldern 1945:134–51). This influential migration theory has been accepted by many scholars, including several still working today.

Palaeoanthropological research in Indonesia has been significant, not only in its own field, but also in archaeology. Systematic excavations and geological studies which were undertaken mainly by the staff of the Geological Service (e.g., von Koenigswald, Ter Haar, and Marks), have resulted in the confirmation of pleistocene stratigraphy especially in Java. Although such stratigraphy was relatively dated, it provided a chronological outline with which to determine the age of palaeolithic artefacts. Concerning palaeolithic artefacts, scholars noticed close similarities between Indonesian artefacts and those of Europe (von Koenigswald 1936).

Clearly, at this synthetic stage, efforts to place archaeological materials systematically into formal, spatial or temporal frameworks were prevalent. In this work, methods and techniques developed in Europe were widely applied. Furthermore, many scholars commonly perceived formal, spatial and temporal variations of archaeological materials as the result of migration and diffusion, explanations which they had borrowed from Europe.

When considering this situation, one might regard Indonesian archaeology at this stage as colonialist archaeology (Trigger 1989:145). Such a notion is possibly true, as most scholars who conducted archaeological research were Europeans who at that time colonized Indonesia. Indeed, the few native scholars played only a small role. In this context, it is possible to say that migration and diffusion theories served to show that native societies had taken no initiative at all in developing their own culture, unless stimulated by a more advanced culture. It should, however, be noted that some scholars, such as Bosch and Leur, were also of the opinion that the indigenous population had played the greatest role in the development of Hindu culture in Indonesia. Whatever stage Indonesian archaeology may have reached by this time, European archaeological thought was clearly influential.

THE AGE OF NATIONAL ARCHAEOLOGY

Archaeological research almost came to a halt during World War II. Dutch scholars were imprisoned by the Japanese, who occupied Indonesia, and Indonesian personnel lacked funds to do research. When the Dutch returned in 1945, Indonesia had already proclaimed its independence and the Archaeological Service had been taken over by Indonesians. Dutch scholars then established their own institution. These two institutions were amalgamated in 1950. Although the new institution was headed by a Dutch scholar, Bernet Kempers, Indonesian personnel now had a greater chance than ever before to carry out research. In 1953, the Archaeological Service was handed over to Indonesians. Meanwhile, archaeology departments were opened in three large universities as sub-units of history departments. The graduates, however, were too small in number to replace the Dutch scholars who had returned to their own country. The shortage of professional archaeologists meant that archaeological research progressed slowly until the mid–1960s when Indonesian archaeologists started to cooperate with foreign scholars from various countries, besides those of the Netherlands.

Such cooperation was clearly beneficial to the development of Indonesian archaeology. Absolute dating, new techniques for collecting data and new approaches were introduced. This acquainted the Indonesian archaeologists with new perspectives for investigating the cultural development of their country. The works of Gordon Childe and Grahame Clark stimulated the development of Indonesian archaeological thought.

Nevertheless, the new perspectives had no immediate impact on the work of Indonesian archaeologists. Prior to 1975 archaeological theories in Indonesia were relatively stagnant. It is true that shortly after independence, there was a strong inclination to support the proposition regarding the significant role of the indigenous population in developing its own culture, as can be seen from the works of Bosch (1961) and van Romondt (1951). They were keen to demonstrate that prehistoric cultural traits were still predominant in both Classical and Islamic culture. This ‘local genius’ proposition is understandable, even nowadays. As a newly independent nation, Indonesia needed ‘something’ which could reinforce its nationalism. Pride in being the descendants of ancestors of genius could partly fulfil this need. It was against such a background that the ‘national archaeology’, which tends to search first of all for evidence for the existence of local genius, usually emerged (Trigger 1989:174). This, indeed, was what happened in Indonesia. Significant as it might be, theoretically the proposition was merely the continuation of the previous proposition of the synthetic stage.

Simultaneously some scholars at this time began to pay attention to the past social life of specific communities. Schrieke (1957) published his two-volume book *Indonesian Sociological Studies,* in which he wrote about the social life of the Javanese in the Classical period. His work was apparently of the kind of

archaeological research which had been suggested by Clark (1969). R.P.Soejono, a prominent Indonesian prehistorian, admitted that he was influenced by Childe and Clark when, in 1974, he constructed a new framework for Indonesian prehistory. He suggested that the socio-economic aspects of prehistoric societies should be the basis for chronological frameworks. By taking this position, he rejected the use of a prehistoric framework based on technological considerations (Soejono 1976). Yet his new prehistoric framework was not essentially different from the old one. Thus, the works of Clark and Childe were still influencing Indonesian archaeology at this time, albeit superficially.

RECENT DEVELOPMENTS IN INDONESIAN ARCHAEOLOGY

As archaeological work, both in preservation and research, increased significantly after the mid–1960s, the Archaeological Service was unable to cope with it all. In 1975 that institution was separated into two independent but cooperative institutions: research and preservation. This separation gave way to rapid growth in archaeological research, as greater energy could now be devoted to it. Multi-disciplinary research projects were organized which included biologists, geographers, geologists and architects. Meanwhile, the archaeology departments in the universities were also making considerable progress. In 1977 a 'Short Course on Archaeological Method and Theory' was held in Jakarta, at which the main materials presented were writings by the American 'New Archaeology' scholars.

At almost the same time the Ford Foundation provided grants to support the development of Indonesian archaeology. Several prominent archaeologists were given the opportunity to study in American universities. Grants were provided for the purchase of foreign books and archaeological journals. American scholars were invited to give lectures in university departments of archaeology and to help conduct research. All of these steps served to stimulate the development of Indonesian archaeology. This new situation has led to the emergence of a new generation of archaeologists in the 1980s, whose perspectives vary considerably. Besides the old culture-historical approach, Indonesian archaeologists are now familiar with the processual and anthropological approaches to archaeology. This is evident in their attempts to implement new theories and methods.

Since 1980 the need for a controlled use of ethnoarchaeology has been underlined (e.g., Mundarjito 1981; Miksic 1986; Tanudirjo 1987), and the need for analysis of taphonomic processes in archaeological research has been emphasized (Mundarjito 1982). Even research similar to Rathje's garbage project has been undertaken (see Faizaliskandiar 1992). The problem of deductive-inductive reasoning has been brought into the open for debate. It was recommended that deductive reasoning should be used more often (Mundarjito 1986). To some extent, quantitative analysis is considered to be prestigious and a more useful tool for data analysis. In relation to the preservation and

conservation of archaeological materials, the idea of cultural resource management has now been widely propagated (Kusumohartono 1986; Tanudirjo & Nayati 1988).

Cultural ecology has been applied to explain the rise of the complex ancient settlement in Trowulan, east Java (Kusumohartono 1985a), the relationship between economic patterns and the political hegemony of ancient Javanese rulers (Kusumohartono 1985b) and the decline of the Islamic kingdom in Banten (Untoro 1989).

Systemic theory has been used to account for the revival of 'prehistoric' culture in the Classical archaeological remains at Mount Penanggungan (Tanudirjo 1986). Systems theory has also been applied to explain the development of pottery technology in a small village near Yogyakarta (Atmosudiro & Tanudirjo 1987).

A trial explanation using an action-based model has been proposed in relation to the subsistence pattern of those who had lived in a Hoabinhian site in east Sumatra (Soekardi 1989). A similar model has served as a basis for modelling the Pleistocene colonization of northern Sahul (Tanudirjo 1991).

Although Indonesian archaeology has been influenced by American anthropological archaeology, this does not mean that the culture-historical approach has been rejected completely. On the contrary, the majority of scholars still apparently prefer to work within this paradigm. So far all dissertations which have been published are culture-historical in their approach. Dutch scholars still undertake research in Indonesia based on their culture-historical archaeology. Although some archaeologists are sent to study in Europe, especially France, current debates there (see Olivier & Coudart, Ch. 18, this volume) do not seem to have had an effect.

Evolutionary theory remains prevalent. The materialist approach is now slowly replacing the mentalist approach used previously. Naerssen (1977), for example, suggests that the shift of the Javanese kingdom from central Java to east Java was dictated by the need for a new strategy to handle the increasing trade in the archipelago. This view differs from the former proposition to the effect that the eruption of Mount Merapi was responsible for the shift, as the Javanese believed that a tremendous natural disaster was the signal that the gods would show no mercy anymore to the kingdom (Boechari 1976).

Migration theory is still in favour, especially in relation to the evidence for the prehistoric inhabitants of Indonesia. Additional support for this theory has been provided by linguistic and archaeological evidence. However, present-day scholars who make use of migration theory do not perceive the migrations that occurred in the archipelago as having consisted of several successive migrations which overwhelmed and replaced the previous population. Instead, such migrations are viewed as the 'slow expansion and adaptation of a relatively unified ethno-linguistic population, combined with inter-group contact and the successive influences of external civilization' (Bellwood 1985:130).

EPILOGUE

From the foregoing it is clear that Indonesian archaeology has a long history. For more than two hundred years, however, this discipline was dominated by Dutch scholars. Indeed, the conduct of archaeological research and the theories applied were greatly influenced by European schools. Only in the last two decades has Indonesian archaeology been acquainted with various new perspectives. The ongoing debate between the culture-historical and processual schools has to be seen as a dynamic process leading towards more scientific archaeology in Indonesia.

In the future Indonesian archaeology should develop its own paradigms to enhance understanding of the development of past Indonesian cultures. The reconstruction of Indonesian culture-history based on an old-fashioned framework, comprising such ideas as the Three Age System, diffusionism and migration should be reconsidered. Since Indonesia is a country of diverse cultures and environments—different from those of Europe—it is unlikely that European theories and models will be wholly applicable. European models will need to be modified and elaborated to make them properly applicable. This is a major challenge for Indonesian archaeologists. The future of their subject depends upon their ability to create, develop and disseminate theories, models and new perspectives of their own. This would enable Indonesian archaeology to contribute more to the development of world archaeology.

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CHAPTER FOUR
THEORY, PRACTICE AND CRITICISM IN THE HISTORY OF NAMIBIAN ARCHAEOLOGY

JOHN KINAHAN

INTRODUCTION

African archaeology is deeply rooted in the history of colonialism, in the protracted independence struggles of modern times, and in the continuing relationships between African states and former colonial powers. According to Trigger (1984, 1989), these complex influences are manifest as distinct archaeologies, nationalist, colonialist and imperialist, which reflect the changing social milieu of research. Although it is in this respect a veritable hall of mirrors, Namibian archaeology does not fit neatly any of these descriptions. As the following history of research shows, there is a thread of ideological continuity which persists in spite of fundamental political change and shifting approaches to the Namibian past.

Since the turn of the century, archaeological research in Namibia has made significant progress under successive German and South African colonial dispensations. But even after independence a large proportion of research is still carried out by visiting scientists from these countries and the archaeological infrastructure of Namibia remains poorly developed. Rock art and related studies are the traditional focus of interest in Namibian archaeology, and in this field the German contribution (cf. Härke 1995) is by far the largest. By examining this research and the alternative approach of South African rock art studies, I intend to show that Namibia is subject to incommensurable competing archaeologies. The long-established tradition of radical empiricism is increasingly under attack from an authoritarian cognitivist approach, and both represent a conventional view of the Namibian past which is vulnerable to an historical materialist critique.

Most of the research activity discussed in this chapter takes place on and around a remote granite massif in the western desert area of Namibia. Once known as Dâures, the ‘burning mountain’ was renamed Brandberg in the course of early colonial topographic surveys. Already then, in the first decades of this century, the mountain was virtually uninhabited and research into its human prehistory took on an unfettered antiquarianism that has exerted a lasting influence on Namibian archaeology.

EVENING ON THE RHINE

In the first week of June 1991 the Landesvertretung Nordrhein—Westfalen hosted at its headquarters in Bonn a select party of influential people, gathered for the opening of an exhibition entitled Weisse Dame—Roter Riese: Felsbilder aus Namibia (White Lady—Red Giant: rock art from Namibia) (Fig. 4.1). The interior of the building had been cleverly transformed to create an arid mountainscape ambience, against which were displayed a large and varied selection of rock art copies resulting from the Namibian research programme of the Forschungstelle Afrika at Cologne University's Institut für Ur- und Frühgeschichte.

Rendered in crisp, inked outline and lifted, as it were, from the original rock, the copies of writhing serpents, animal-headed people and strangely disordered friezes, gave two simultaneous but contradictory impressions. On the one hand, the association, juxtapositioning and conflation of people and animals denoted a clearly primitive artistic tradition, while on the other, the skill and faithful precision of the copies established the advanced scientific standard of the investigation. This contrast served at once to acknowledge the beauty and mystery of the art, and to shift it from the rocks to the copies, so defining the relationship between scientist and subject-matter.

Some, though not all of these considerations emerge in the text of the exhibition, where Kuper (1991) is at pains to argue that the rock art represents the unwritten history of Africa. He contrasts the research presented in the exhibition with that of earlier investigators who viewed the rock art as mere pictures and not as the *einzigartige historische Dokumente* (unique historical documents) which they really are. It is this claim to a more progressive understanding that Kuper wishes to convey in the title of the exhibition itself. 'White Lady—Red Giant' opposes the myth of Mediterranean origins once used to explain the finer paintings, with the pre-eminence of an indigenous African origin which Kuper proclaims by inventing a counter-myth, based on the connotation of size and strength in the Red Giant. Self-evidently, this device has no real basis in the rock art which the text goes on to introduce through the history of research by a succession of determined and unfailingly thorough investigators. Their research, the viewer is told, unlocks the archive of the new Namibian state, uncovering the roots *(Wurzeln)* of national integration and identity.

On proceeding further into the hall of the exhibition, these intriguing sentiments receive less direct emphasis at first, being set aside to consider the methods and techniques of scientific research in this field. The rather poor quality of earlier copies immediately establishes beyond question the authoritative basis of the Cologne research, shown to exemplify the highest attainable standards, even in the gruelling field conditions of Namibia. Guided thus, some of the more obvious characteristics of the art become clear to the viewer exposed to the genre for perhaps the first time. The various animals are

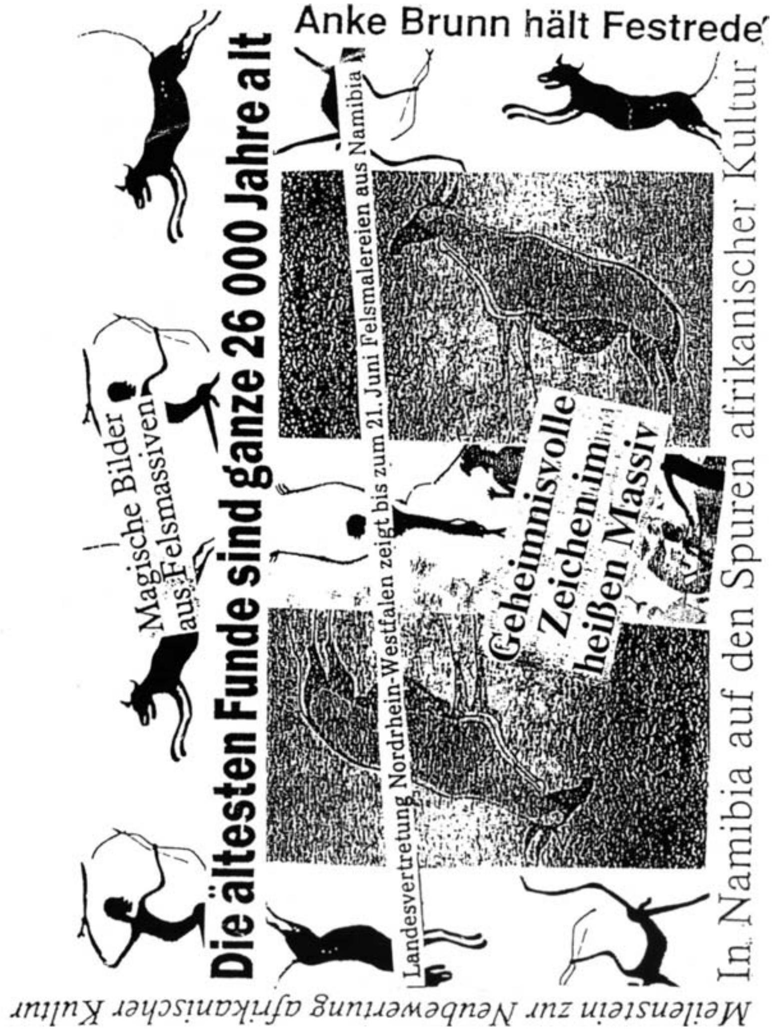


Figure 4.1 The exhibition *Felsbilder aus Namibia* held at the *Landesvertretung Nordrhein-Westphalen,* Germany, June 1991

identified to species and their habits explained with reference to the paintings. Human figures are numerically predominant in the rock art and the text draws attention to the range of activities shown, including hunting, dancing, camping and various domestic scenes. Skirting the question as to the motivation of the artists, the text moves on to explain the procedure developed to analyse the meaning of these seemingly complex and difficult friezes. A simple typology is the first and archaeologically sound step by which the paintings are brought from riot to order, although without disturbing obviously meaningful associations. The

method subjects these to a sort of structural linguistic analysis by which the meaning of the art is revealed, disappointingly, as a series of banal, stacatto sentences, like giraffe walks, woman carries bag, man holds bow. Having arrived at this point the viewer is greeted by a soundtrack of insistent drumming accompanied by flashing images of the rock art, brought to life in bizarre, marionette-like animation.

Turning away, the viewer is drawn to the beat of a different but complementary rhythm produced by a group of live performers brought from Namibia for the occasion. Their raw energy and evidently simple enjoyment is presumably to enliven the exhibition of the rock art, and to link it with a colourful selection of photographs showing the joyous culmination of Namibia's struggle for political independence. It is indeed one of the more problematic aspects of the Bonn exhibition that it presents the rock art as if the Namibians could do no more than sing its accompaniment.

THE WEST AFRICAN SPEAR

The attention of outsiders was first drawn to the Namibian rock art in the late nineteenth century (Palgrave 1879) and reported thereafter with increasing frequency as colonial officials began to explore their new domain (Moszeik 1908; Jochmann 1910). Lasting distinction is however reserved for Reinhardt Maack, whose rock art discovery in 1917 has become a minor classic in the history of Namibian archaeology (Maack 1960).

All but overcome by thirst and fatigue in the remote Tsisab Ravine, Maack crawled under a massive boulder to rest and awoke later to find on the face before him an extraordinary and delicately painted composition of animals and people, apparently in procession. Maack had become separated from his companions and after failing to draw their attention with an excited volley of shots, he copied as best he could the elegant figure at the centre of the frieze, and hurried on. Eventually Maack's crude sketch reached the Abbé Henri Breuil, who records that years before he was able at last to visit the site he had perceived the great significance of the figure and named it the White Lady (Breuil 1955:2) (Fig. 4.2). It is a curious fact that although Maack identified the figure, correctly, as male, the Abbé never doubted that it was female, even when he examined the frieze in person (Fig. 4.3).

Arriving at the foot of the Brandberg massif, where the Tsisab Ravine leads up to the site, Breuil had 'the impression…of a great fallen acropolis or palace… between the granite slabs and boulders there are flat sand-covered surfaces like squares or courts between dwellings' (Breuil 1955:5). In the Maack shelter itself, Breuil found a complex palimpsest of fully eleven separate episodes of painting. The first six he quickly dismissed as 'miserable' precursors of an intermediate stage, over which is painted the final 'symbolical ceremonial procession' (Breuil 1948:4–5), featuring the White Lady and twenty-six accompanying figures. In his examination of this group Breuil drew particular attention to the appearance



Figure 4.2 The White Lady as traced by Harald Pager (with permission of the late Harald Pager)

of clothing and accoutrements, telling indications of exotic affinities. 'Rosy white from her waist to her feet', the White Lady 'wears a clinging garment rather dark coloured from her waist to her neck, with short sleeves and several beaded bands, such as her companions wear, at knees, hips, waist and wrists. Like several of her neighbours she has various objects stuck into her armlets. The face is very delicately painted and has nothing native about it' (Breuil 1948:6–7).

The specific foreign influences in the paintings were mainly identified by Mary Boyle, Breuil's assistant and an authority in her own right. Apart from the gracile features of the main figure, Miss Boyle remarked on details of dress and

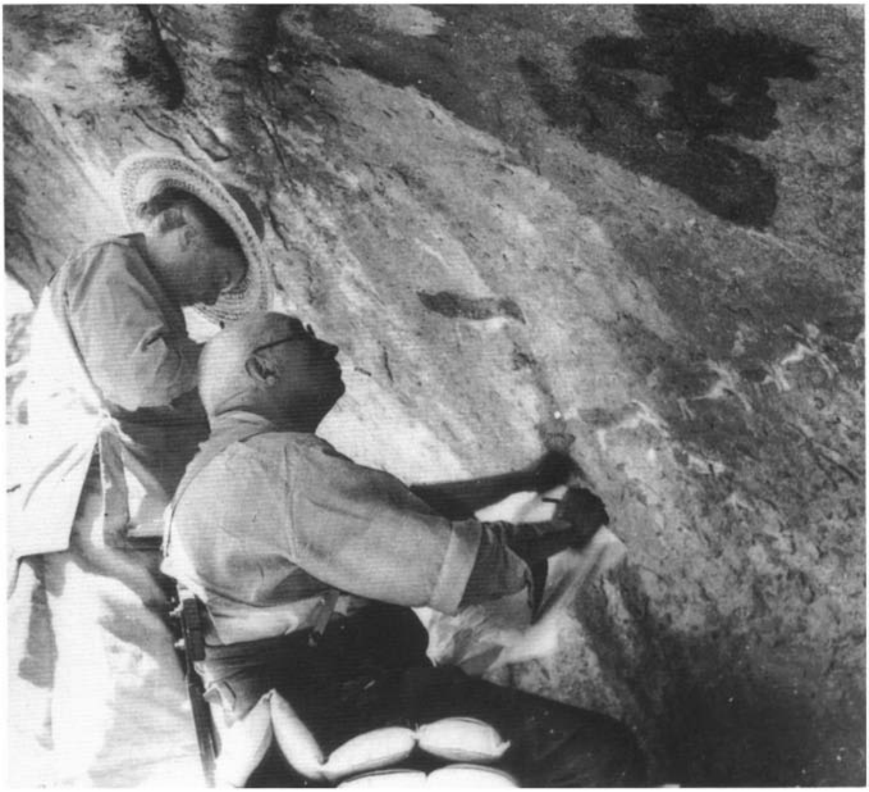


Figure 4.3 Abbé Breuil and Miss Boyle at work on the White Lady frieze (Photo: E.-R.Scherz, reproduced with permission from the National Archives of Namibia)

the apparent relationships of the various attendants and other figures in the cortège, all replete with allusions to Crete and Egypt. There could be little doubt that the frieze depicted Isis herself, in the Lesser Mystery of Egypt (Breuil 1955: 21). This confident assertion explained the prominence of a female figure in the art of a supposedly uncivilized region, for clearly the frieze was evidence of a hitherto undocumented journey into the African interior. Breuil's patron, Field Marshal Jan Smuts, the South African Prime Minister, seemed greatly pleased by these observations and when his administrator in Namibia, Colonel Imker Hoogenhout, arrived at the White Lady, he reportedly turned and said, 'You are absolutely right, this is no Bushman painting: this is Great Art' (Breuil 1955:7).

Although he was something of a celebrity in the capital of colonial Namibia—Breuil even sat for the noted portrait artist, Otto Schroeder (Fig. 4.4), himself a keen student of the rock art—most southern African scholars rejected his extravagant interpretation of the White Lady frieze. The most prominent and damaging critic was Schofield (1948), who argued that the correspondences with

Cretan and Egyptian art were entirely deceptive and fortuitous. Stung to reply, Breuil (1949) could only insist on the necessity of what Schofield had dismissed as the 'lumber of pseudo-antiquity' (Schofield 1948:86). The growing literature on southern African rock art showed less and less inclination to venture beyond what appeared to be indisputable facts: that many of the paintings were relatively recent (Walton 1954:5), being the work of extinct Bushman groups (Clark 1959: 280). The general opinion was that the art merely represented scenes of everyday life, although some examples were obviously related to arcane, lost rituals and beliefs.

However, if Mediterranean interventions were to be denied, so too was African creativity in the unsteady balancing act attempted by the *South African Archaeological Bulletin,* which decided that 'indisputable exoticism is of far greater importance than some chance suggestion of similarity' (editorial: 1951: 3). In the editor's view, the Abbé and his critics were both correct, since the dress of foreigners would have been imitated by Africans and they, in turn, would have been painted by Bushmen; such 'are things we complacent residents have missed'. Breuil had become president of the South African Archaeological Society in 1947 and his stature was unquestioned; there were no further heated exchanges over the authorship of the rock art. It is therefore curious and significant that on leaving Namibia for the last time Breuil gave to Ernst-Rudoph Scherz, an amateur enthusiast, the West African spear he had carried throughout his desert journeys, saying as he did so: 'Je vous rends les armes.'

The close association betwen Breuil and Scherz is played down by Breunig (1986:55), who states that Scherz began his magisterial three volume survey of Namibian rock art from a list compiled by Maack (1921). Although Breunig presents Scherz as the lineal successor of earlier German scholars (Lebzelter 1930; Obermaier & Kühn 1930; Frobenius 1931), it was only after Breuil had left, as of the early 1960s, that this work received regular support from the Deutsche Forschungsgemeinschaft and the University of Cologne. Furthermore, the commencement of Scherz's studies marked a clear departure from the developing trend of related research in southern Africa, and contributed substantially to the rise of what Lewis-Williams (1990) has termed the 'Cologne school'.

In contrast to the interpretative studies of Breuil in Namibia, the abiding purpose of the Scherz research was to compile 'a corpus of evidence which would form a sound basis for future, detailed regional studies' (Breunig 1986: 55). Accordingly, Scherz travelled the length and breadth of the country, documenting more than four hundred sites. Each site was described and catalogued; the paintings were photographed and where necessary, traced and reproduced in watercolours. The whole exercise was self-consciously technical, in that Scherz avoided all speculative interpretation, striving for the highest possible degree of objectivity. Unlike Breuil, he apparently had little faith in the use of superpositioning as a means to determine relative age. Yet since the paintings themselves suggested a considerable span of time in their execution,



Figure 4.4 'Abbé Breuil, Membre de I'Institute', by Otto Schroeder (Photo: author; courtesy, Archaeology Laboratory, State Museum of Namibia)

the project naturally led to the investigation of dating by physical means. Denninger's age determinations based on paper chromatography of albuminous binders in paint samples, indicated that most of the Namibian paintings were more than 800 years old (Willcox 1971; Breunig 1986). It was nonetheless

surprising when the archaeological excavation programme of the project yielded a series of painted plaquettes dating to approximately 26,000 years BP, the oldest dated works of art in Africa and comparable in age to the paintings of the Upper Palaeolithic in Europe (Wendt 1972; Wendt 1975). This discovery lent considerable impetus to the Cologne research programme and led directly to the mounting of its most ambitious project: the documentation of the rock art of the Brandberg massif, largely uninvestigated since Breuil, but widely acknowledged as the major Namibian concentration of painted sites.

Early investigations of the Brandberg by Mason (1955) and Rudner (1957) had helped to establish the indigenous associations of the rock art and thereby clarify the questions as to its authorship, but the extreme ruggedness of the mountain terrain had effectively discouraged more sustained research (Wendt 1972:5). When Cologne University engaged Harald Pager in 1977, he believed it possible to document the rock art of the Brandberg within two years, but the project was still incomplete when he died in 1985, having recorded a total of 879 sites out of 1, 045 located during his survey. As with the work of Scherz, completed posthumously by Breunig (Scherz 1986), that of Pager is to be published in full and one volume of the intended series has already appeared (Pager 1989).

The Pager volume presents a very clear articulation of the goals and premises of the Cologne research which still espouses the same empiricist approach today as its earliest amateur fieldworkers more than thirty years ago. Kuper, in his foreword, states that a ‘complete and exact documentation…constitutes the prerequisite for any kind of interpretation’ (Kuper 1989:13), a problematic view which takes little account of recent progress in southern African rock art interpretation and has therefore attracted strong criticism (Lewis-Williams 1990; Dowson 1990). Advances in southern African archaeology since the 1960s laid down a firm basis for the understanding of hunter-gatherer subsistence and settlement patterns during the last 10,000 years (Deacon 1990). Although this research made systematic use of ethnographic sources, rock art was considered until recently an unreliable form of evidence, difficult to date with any precision and patently misleading in its often unrealistic representation of animal and human life. For these reasons southern African rock art remained largely in the province of the amateur until the pioneering studies of Vinnicombe (1976) and Lewis-Williams (1981).

Of central importance to the approach developed by Lewis-Williams is the role of trance performance in southern African rock art. Having conclusively demonstrated specific correspondences between rock art depictions and ethnographic descriptions of ritual trance performance, Lewis-Williams (1981, 1982) argued that the paintings are so intimately related to ritual activity that they cannot be well understood from any other perspective. This is particularly apparent from the observation that the trance ritual incorporates certain animal metaphors which occur repeatedly in the rock art. Earlier workers sometimes viewed the paintings as a form of sympathetic hunting magic, or as art for its own sake, but it is now widely accepted as the result of Lewis-Williams’ studies

that since the rock art of southern Africa has its origin in the supernatural experience, the content of the art is therefore never self-evident. One major consequence of this view is that it decisively rejects empiricist claims to a valid description of the rock art.

The appearance of simplicity and straightforwardness that empiricism brings to the rock art is deceptive, for it imposes a set of premises which are not only irrelevant to the paintings, but inherently flawed to the extent that they are bound to mislead the process of research. Lewis-Williams & Loubser (1986) show that although data collection, the supposed first stage of any empiricist investigation, is fraught with problems of selectivity, these are seldom addressed and observations are often collated in the mistaken belief that they constitute raw data. Similarly, the process of classification approaches the data as if no sorting had occurred in the process of its collection, and as if ambiguity could be avoided by strict measures of objectivity. The final stage of empiricist research, induction, very frequently ignores the extent to which the data merely reflect the investigator's own prior assumptions. Although, as Dowson puts it 'critiques of… empiricism have become almost as tedious as empiricism's continued practice' (Dowson 1990:172), the radical, anti-theoretical stance adopted by the Cologne research effort in Namibia (Kuper 1989) is not to be lightly dismissed. The lengthy criticisms of Lewis-Williams (1990) identify grave weaknesses in the Cologne programme, but do not address its consequences for Namibian archaeology. There is indeed some common ground between these adversaries, for both apparently exclude the potential of the rock art as an historical record.

NAMIBIA AFTER KOSSINNA

According to the conventional account propagated by Heinrich Vedder (1938), pre-colonial Namibia was a maelstrom of ethnic rivalries and incessant tribal warfare. This influential view informed many decisions of colonial policy, and supported the belief that it was only the intervention of Germany at the end of the nineteenth century which brought a measure of peace, reinforced by the separation of communities into discrete ethnic homelands. Under South African rule following the Treaty of Versailles, this specific policy provided one of the major unifying issues in the struggle for national independence (SWAPO 1981). Historical and ethnographic studies played a modest but essential role in the justification of tribal land apportionment (Esterhuyse 1968; Goldblatt 1971) and although some systematic critiques have appeared (Drechsler 1980; Lau 1981; Fuller 1993), archaeological studies generally accept historical ethnographic studies as a valid reflection of the more distant past. Most archaeological studies in Namibia tend to equate artefact assemblage with ethnic identity, and assemblage sequence with ethnic succession (Wadley 1979; Jacobson 1980a; Shackley 1985). This approach leaves unquestioned the conventional view that the cultural diversity of pre-colonial Namibia is the cumulative result of successive invasions from the north (cf. Stow 1905; Vedder 1938), rather than

any other process which might result in fundamental social and economic change.

Racial and ethnic determination of archaeological remains began in Namibia with the work of Lebzelter (1930) and became firmly established with that of Rudner (1957) and Viereck (1968) whose work in the Brandberg convinced them that recent stone artefact assemblages were too crude to have been made by the same people as the rock art. Rudner's postulated ethnic succession was taken up by Jacobson (1980b) as an explanation for change in the archaeological sequence, using the argument that the artists must have been displaced by the arrival of pastoralists whom he claimed were depicted in sufficient detail in the White Lady frieze to allow the identification of their specific ethnic group from items of clothing and ornamentation. The archaeological basis of these claims is insubstantial (Kinahan 1991), and that particular interpretation of the rock art inadvisable, since colouration and decoration of human figures in the rock art are now known to reflect artistic conventions of symbolic value and not 'a kind of ethnographic scrapbook' (Lewis-Williams & Loubser 1986:255).

Although the Cologne research on the Brandberg rock art is the most comprehensive to date, Pager (1989) and his various contributing authors, including Kuper, Breunig and Lenssen-Erz do not address these problematic issues or any related matter of broad theoretical concern, other than to state that contrary to the arguments of Lewis-Williams (1983:3) interpretation must necessarily await full documentation. In this way, the institutionalized theoretical void in modern German archaeology (Härke 1991; Härke 1995) exerts a strong retarding influence on the progress of research in Namibia, an unexpected consequence of the aptly-named 'Kossinna Syndrome' (Smolla 1980). Furthermore, deferring debate over the validity of earlier research also has the effect of mystifying the Namibian past as expressed in the rock art, and not opening it to a multiplicity of alternative views, as is intended by Lenssen-Erz (1989).

Unfortunately for Namibian archaeology, the cognitive approach to the rock art pioneered by Lewis-Williams is also problematic in that its essentially ahistorical conception of pre-colonial society offers the archaeologist no alternative to the ethnic-centred views of earlier workers. Through the symbolic content of the rock art and its relation to ritual healing, Lewis-Williams (1982) made clear the integration of these practices with the hunting and gathering economy of the artists. Far from being a remote, esoteric domain, ritual activity, and therefore painting, closely reflect social conditions as they, in turn, were affected by environmental and other conditions. Although historical events which led to the demise of a hunting society in large parts of southern Africa are rarely intimated by the rock art, the occasional depictions of conflict with Bantu agriculturalists or European settlers have supported the view that the hunters were generally marginalized or exterminated (Wright 1971; Parkington 1984). Evidence of extraordinary continuity in the rock art, linking the late Pleistocene plaquettes from Namibia with more recent depictions, is taken by Lewis-Williams

(1984) to confirm an apparent lack of evidence for social evolutionary changes in southern African hunting society. Thus, while the indigenous artistic and intellectual achievement of the rock art is to be emphasized, even celebrated (Lewis-Williams 1990), its practitioners are committed to an ethnological limbo.

Lewis-Williams (1984) has argued that the specialized function of the rock art must suppose the existence of a specialist practitioner, the shaman. This view is partly based on the plentiful evidence of shamanism in nineteenth century ethnography, to which Lewis-Williams (1984) accords sufficient authority over the distant past to claim that shamanism is a definitive feature of the rock art, irrespective of its age. Consequently, the cognitivist approach, in recreating the spiritual life of southern African hunters, tends to become an end in itself (cf. Shanks & Tilley 1987:84), apparently unable to admit or evaluate contrary evidence from the archaeological record which points to fundamental economic changes accompanying the widespread establishment of nomadic pastoralism in the subcontinent during the last two millennia.

New research in the Brandberg and surrounding region does however offer a possible solution to this impasse by combining rock art interpretation and more conventional archaeological techniques (Kinahan 1987; Kinahan 1991; Kinahan 1993). Within one of the larger concentrations of rock art, two major sites had well-stratified deposits with broadly comparable occupation sequences dating from about 4,500–500 years BP. Throughout most of the sequence, until approximately 1,000 years BP, faunal remains indicated that subsistence relied heavily on snared game, including hares and small antelope. The accompanying stone tool assemblages and other artefacts such as worked bone and shell, as well as leatherwork, showed very little change during the course of these four millennia. In both sites, pottery first occurred at around 2,000 years BP, as a relatively scarce import; one site yielded a small cache of trade goods including iron and copper beads and cowrie shells associated with the final occupation of the shelter at about 500 years BP, when it was used as a sheepfold. Shortly thereafter, both sites were abandoned and hunting and gathering based at the rock art sites gave way to nomadic pastoral settlement, represented by the remains of small encampments with enclosures for livestock. The evidence from this investigation does not confirm the view of earlier researchers that hunting communities were displaced by immigrant pastoralists. On the contrary, the evidence of an indigenous transition from hunting to pastoralism shown by the excavation results (Kinahan 1984; Kinahan 1986; Kinahan 1991) prompts a reconsideration of the rock art.

In the prolific rock art of the two excavated sites there were many examples of superpositioning, such as is characteristic of the Brandberg as a whole (cf. Rudner & Rudner 1970; Pager 1989). Groups of monochrome red human figures are a consistent feature of these sites which often show lines of dancing men accompanied by clapping women. Sometimes the men support themselves on short sticks, as if in imitation of animals; some men are shown in attitudes of collapse, occasionally with blood streaming from their noses. The

correspondences with ethnographic descriptions of the trance dance are well established and such paintings clearly refer to the common tradition of ritual healing and solidarity in southern African hunting communities (Marshall 1969; Marshall 1976; Lewis-Williams 1981; Lewis-Williams 1982).

Superimposed on the monochrome figures is a more recent episode of painting which includes elaborate polychrome men. A characteristic of these paintings is the emphasis placed on areas of the body such as the head, shoulders and waist with rows of fine white stippling or blocks of colour. Usually painted as single figures, the polychromes indicate a degree of specialization and male exclusivity which is in contrast to the earlier monochrome groups, although clearly they are a development of the same artistic tradition. These indications of a shift in the ritual practice associated with the rock art were overlooked by earlier workers such as Breuil (1955), who mistook similar features for the fine clothes of a Mediterranean girl, while Jacobson (1980b) supposed them to be the traditional beaded ornaments of Herero pastoralists. Indeed, the infamous White Lady frieze well exemplifies the evidence favouring an alternative view of the Namibian rock art: the fine stipplings and some of the other markings on the polychrome figures probably represent the supernaturally charged perspiration, used by the shaman in ritual healing, and the accoutrements of these particular figures include fly whisks and dancing rattles, basic items of the shaman's equipment.

The change implied in the superpositioning of the rock art cannot be directly dated, but it is reasonable to propose that it is linked to the only major change to occur in the last five millennia, the evident shift from hunting and gathering to a fully pastoral economy. An illuminating and helpful ethnographic parallel is to be found among the Nahron, a southern African hunting society in which healers became ritual specialists, or shamans, whose skills were highly valued by neighbouring pastoralist communities. Paid for their services in livestock, these men became wealthy in their own right (Guenther 1975), effectively contributing to a transformation of the hunting community. These observations and the evidence from the Brandberg sites support the argument that shamanism arose in a southern African hunting society as a response to changing economic circumstances and that the role of the shaman as an agent of social change is reflected in the rock art (Kinahan 1991). In this view, the art is not a passive reflection of social conditions, but a product or consequence of active intervention by the artist and shaman, manipulating metaphors and images to resolve basic contradictions between the egalitarian ideology of a hunting society and the economics of pastoralism. The sequence of abandonment of the rock art sites suggests that shamanism may have been a short-lived phenomenon, serving only to establish the nucleus of livestock ownership which formed the basis of the pastoral economy.

CONCLUSIONS

The history of archaeological research in Namibia, being essentially colonial in character, is similar to that of many other African countries (cf. Robertshaw 1990). However, the unusual circumstances of colonial rule by Germany and South Africa successively have influenced the development of Namibian archaeology in a number of ways. Most important, there is a measure of agreement between the premises of archaeological research as described here, and the ideology of colonial government. But the relationship is complex and merits further discussion, if only to establish that the critique of colonial archaeology does not in this case presage some form of nationalist archaeology in Namibia (cf. Ki-Zerbo 1989).

As this history of research has shown, the main growth of German archaeology in Namibia took place after the establishment of South African rule and more especially in the period since World War II. In this respect, Namibia provides an interesting and previously unnoticed example of the post-war crisis in German archaeology (cf. Härke 1995) and its effects on archaeological research in a former colonial possession. Of particular interest is the continuing colonial relationship between German institutions and Namibian archaeology, as illustrated by the Bonn exhibition described above, and by the fact that the results of the German research reside in Germany and not in Namibia, as is the case with other former colonial powers (Ucko 1993). Although the German research has contributed significantly to the documentation of Namibian rock art, criticism of the empiricist approach has mounted in southern African rock art research which currently favours a cognitivist approach, incommensurable with empiricism.

Advocates of this alternative have demonstrated that Namibian rock art is amenable to the same methods of interpretation now widely adopted throughout southern Africa. Significant though it is, the cognitivist advance offers no means to investigate the validity of conventional views on the processes which shaped pre-colonial Namibian society. Without some relationship to the archaeological record as a whole, the detailed understanding of the rock art provided by the cognitivist approach is inevitably ahistorical. Fortunately, evidence from the most recent Brandberg rock art research and excavations provides an alternative view of the sequence in this area. Continuity in both subsistence technology and ritual practice suggests gradual change rather than ethnic succession as favoured by earlier research, while the evidence of long-distance exchange suggests a means to the acquisition of livestock from elsewhere. Changes in settlement patterns indicate a successful transition from hunting to pastoralism. Against this background, a plausible explanation of the observed change in the rock art is that it relates to an ideological shift represented by the rise of shamanism, a process which is not as readily indicated by other kinds of archaeological evidence. Such evidence was either mistaken or unobserved by previous researchers.

The initial South African patronage of archaeological research in Namibia under the Abbé Breuil reflects a fascination with ancient antecedents of colonialism, as described by Trigger (1984), but official interest soon declined and the White Lady retired among the trivia of settler mythology. Archaeologists, having demonstrated the indigenous associations of the rock art, sought to explain apparent changes in the sequence by relating them to Vedder's (1938) conventional history of Namibia, an apologia for colonial rule and basic article of faith in some way exempt from testing against the results of excavation. For example, the alternative interpretation of the Brandberg sequence as showing a local transition to pastoralism, rather than migration and succession (Kinahan 1991), is criticized by Webley (1992:27) for failing to specify the ethnic group of the peoples concerned, notwithstanding that this supposedly essential result of archaeological research can only be obtained from colonial ethnography which significantly postdates the archaeological evidence. The continuing preference for racial and ethnic explanations over those which consider social or historical processes is well exemplified by Smith (1992:88) who declares, in an ironic echo of Breuil, that one painting from the Brandberg being negroid in appearance is evidence for ethnic migration. The same painting and the complex frieze of which it is part, have been shown to point to the rise of shamanism as a more likely explanation and this is supported by the results of excavations at the same site (Kinahan 1991). Smith (1992) ignores this alternative to the conventional interpretation although an earlier review by Maggs & Whitelaw (1991) had stressed its significance for the archaeology of nomadic pastoralism in southern Africa.

This chapter would be incomplete if I neglected in my conclusions to address the considerable gap which exists between archaeological and other historical research in Namibia today. Archaeology in Namibia, as elsewhere in Africa (Trigger 1984), has concentrated mainly on the more distant, Stone Age past which is of little direct relevance to modern historical events. There is, however, more than just the passage of time that separates the archaeological and historical past. In the first place, direct historical continuity between the rock art sites and modern communities has been broken, by real social transformations such as I have described, and by the imposition of ethnic labels on different components of the archaeological record. Furthermore, there is no particular link between the past as it is known through archaeological research and the modern leadership of post-colonial Namibia. It is therefore understandable, but nonetheless interesting, that the studies of historians exiled during the liberation struggle offer no critique of conventional views on precolonial Namibia (Mbuende 1986; Katjavivi 1988) such as I have discussed in this chapter.

Indeed, if the factitious history which served the interests of colonial government can also serve the interests of post-colonial politics (Vail 1989:3–18; Fuller 1993), then the critique of colonial archaeology presented here should be considered as a dialogue within colonial archaeology (cf. Shanks & Tilley 1987:84), and not as the beginning of national archaeology in Namibia.

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CHAPTER FIVE
EUROPEAN ENCUMBRANCES TO THE DEVELOPMENT OF RELEVANT THEORY IN AFRICAN ARCHAEOLOGY

BASSEY W.ANDAH

There is no doubt that African archaeological traditions of theory and practice have been influenced by European archaeological traditions and theories. This is largely the result of European colonization of various parts of the continent. The principal European influences have been British, French and German (in Namibia), because although the Portuguese, the Spanish and Belgians also participated in the colonial partition of Africa, and practised some archaeology in their colonies, their own traditions of archaeology have been largely derived from, and dependent upon, those of the other more dominant European powers.

The European archaeologies in question clearly did influence African archaeology in more ways than one, yet in many respects these influences have at best constrained rather than aided or facilitated a proper understanding of African cultural history. More often than not they actually hampered such understanding by providing systems and models and practical methods of study which were in many cases inappropriate for the study of African peoples and cultures. Before trying to construct an appropriate framework for discussion I will first indicate my own understanding of what archaeology is, and then examine and discuss European influences within this context. In particular I will discuss the specific type of impact colonialism had on Africa as compared and contrasted with its impact on other parts of the colonial world.

SOME IMPORTANT ISSUES FOR ARCHAEOLOGY

Archaeologists need to recognize that different people are likely to have different impacts upon the landscape and it is essential to be able to interpret the varied stories of such impacts. The archaeologist therefore has to be expert at reading the language of landscape dynamics, that is, be able to distinguish the effects on landscapes of human occupation from the impact of natural forces (such as water and wind), and the interaction between these over time.

Archaeology can be understood as the study of the interaction between context (i.e. natural landscape) and text (i.e. cultural landscape), both within specific time-limits and through time. Archaeology is the search to identify and understand how these two landscapes relate, and what stresses, tensions and conflicts, if any, exist between them.

Archaeology can also be viewed as the science or art of reconstructing the way peoples themselves constitute and actualize culture, and the use made by such peoples, at various stages in time, of such information about their own pasts. In this respect, archaeology can be seen as (1) the study of cultural norms as formulated and perceived by the people themselves, rather than as the study of the exotic and bizarre, which is what has usually fascinated (imperialist) outsiders; (2) the use made by peoples of their pasts for shaping the directions of their futures—by determining what is worth preserving, what should be conserved and developed; and (3) the discipline which constitutes the basis for dialogue and meaningful communication between human groups.

Where archaeology is properly conceptualized and practised, it will therefore comprise scientific and technological information relevant to a people's charting of their own progress; socio-economic information (institutional and other) and artistic information useful to them as a basis for understanding themselves as scientists, technologists and artists, nurturing their artistic temperament and heritage. There is thus a level of actual reconstruction of a people's past to which archaeologists must be faithful, and a limit to the amount of creative imagination in which archaeologists can indulge. In practice, however, the delimitation of cultural artefacts, features and regions in archaeological research into Africa's past had often little or no real bearing on the *socio*-cultural entities being investigated, perhaps because Africans—the social actors producing culture—have not normally been the central concern of archaeologists. The delimitation of cultural structures and forms and spatial and temporal boundaries has either been carried out in an arbitrary way or on the basis of natural geographical features or, at best, on the surface spread of features and/or artefacts seen, from a European angle, to possess very distinctive styles.

THE PRACTICE OF ARCHAEOLOGY IN AFRICA

These criticisms apply in particular to archaeological studies of African sites and regions which have utilized a normative cultural concept rather than a processual one for characterizing their findings. Such studies define cultural entities in terms of purported *typical* artefacts and *typical* assemblages retrieved from a site, and which are believed to be more or less replicated in their entirety from nearby sites in the contiguous geographical region. Such an approach to interpretation not only depends on the assumption that artefact traits can be equated with cultural entities, but allows easy manipulation to portray prehistoric and historic peoples of Africa as, at best, able only to adapt to environmental changes and hazards. Sometimes such monstruous cultural constructs have been credited with ethnic status, as in the case of Nubia and the Early Iron Age of central and southern Africa (e.g., Wendorf 1968). Migrations and diffusion have been used to account for the history of very large regions of Africa (and for a critical review, see Sinclair, Shaw & Andah 1993).

Directly or indirectly, western social sciences, including archaeology and history, have been—and continue to be—an extension of a power system that seeks to impose its will, as well as its socio-economic and overall cultural system, as the one valid worldwide system. Whether consciously or not, the relationship of western scholars to their subject of study (i.e. African peoples) has usually been *an unequal one* and the information gathered (from local ethnographers, oral historians, archaeological sites and artefacts, etc.) has hardly ever been obtained in a fair and open climate devoid of pressures. Usually the trained western researcher has occupied the driver's and not the learner's seat, and so usually *guides* and *shapes* what his informants, living or dead (e.g., abandoned sites and artefacts) reveal. Archaeologists, anthropologists and historians start out from European concepts and standards, not those of African society, when determining which conceptions of culture, civilization, community, settlement time, history or other basic categories are to be viewed as valid, and which should be discarded (Grosz-Ngate 1988:501). Too often such researchers simply assume that they are privileged to 'belong' to the only 'scientific' cultures in the world—unlike their informants (who are 'illiterates')—and they assume that they are qualified to separate precise facts from 'vague and confused' local traditions (e.g., Goody 1977). The knowledge derived from informants has to be 'evaluated, appropriated and classified by a representative of a voracious and systematizing Western power/knowledge system' (Grosz-Ngate 1988:501). Virtually all purveyors of this western knowledge system believe that it is their sole business (or responsibility) to *construct* the history of Africans.

Even when the study of African ways of thinking, speaking and doing things is well intentioned and broadly conceived, the outcome has usually been lacking in detail, and reductive. This is all but inevitable if the task of describing such a vast variety of cultures, societies and civilizations is to be manageable and the results are to be 'useful' for western *policy* formulation. Largely for this reason also, the *principles* underlying indigenous customs always have to be forced into categories derived from western social reality. This categorization has led among other things to the emergence of several dichotomies between Africa and the West regarding cultural and social forms such as: property, contracts, marriage and the family, social organization, urban and rural civilization, the state and religion, general cultural values, historical and archaeological cultural categories (mainly material), forms, formation and transformation processes.

ANALYSIS OF AFRICAN SITES

Many western studies of African archaeological sites appear not to have benefited from the many and varied historical and ethnographic examples of African modes of manipulating cultural landscapes. A principal source of major concepts used in these admittedly trail-blazing Early Man studies was non-human, rather than human, primate studies—witness such terms as home range, home base, food sharing. This school of research has tended to dwell

disproportionately on those odd relics of the continent, in particular the so-called pre-industrial societies—of foragers, primitive farmers and herders. Visual representations, especially of Early Man types, bear this out, as the earlier hominids are usually represented as black, while *Homo sapiens* is usually depicted as white. The impression given is that Africa has always been—and continues to be—peopled mainly by hunter-gatherers and primitive peasant folk. For many western scholars Africa has been at best a classic laboratory for studying human origins, early cultural evolution and, in particular, Pleistocene foraging hominids from a clearly European rather than African cultural standpoint.

There are also archaeological studies—albeit so far very few—of later periods of African cultural history (e.g., McIntosh & McIntosh 1984, McIntosh & McIntosh 1993 for Jenne-jeno; Spear 1989 for Swahili cultural history) which not only attempt to relate the definition of research area directly to the cultural grouping or problem being investigated, but also try to utilize all relevant sources (oral, linguistic, historical, written, ethnographic) in an effort to decipher the course of history of either a people or a settlement or settlements. However, even in such studies there are unsatisfactory theoretical postulates or assumptions: (1) that an African settlement, and in particular every early African town, in order to be adjudged as having developed, would need to have been within the ambit of a central place, manifesting the same type of hierarchical structure as those which have been developed in European societies; and (2) that there should be an equation between natural/ecological region with cultural region.

In the absence of solid empirical information, the tendency, given the prevalent traditions of archaeological and related historical research, has been to impose an evolutionary cultural sequence on Africa's past and quickly to assign cultural labels to site finds and their features and artefacts, as soon as supposedly diagnostic artefacts have been found. In a very real, even if subtle, sense researchers have often been constrained to work within a predetermined framework. Thus, for example, features and/or artefacts found or associated with supposedly diagnostic ones, are never accorded their due importance, no matter how different or how numerous. Similarly, researchers are obliged to search for specific 'known' entities (such as Acheulean, Sangoan, Lupemban, Levalloisian, etc.), even when these are mirages. Such an approach drastically reduces the possibility of discovering features and entities characteristic of, and sometimes unique to, particular regions of Africa, and may even rule it out completely. The effect is to paint historical Africa as bereft of social life and social thought, and to represent Africans as always shaped by the environment. At best Africans are represented as adapting periodically to deteriorating climatic conditions by migrating, or as the recipients of new technologies and social constructs deriving from outside (e.g., McIntosh & McIntosh 1983; Klein 1983).

Such reliance on untested and preconceived notions, together with the concentration on the study of cultural traits and their distributions, has encouraged any cultural achievements evident in the African record to be

credited to outsiders (e.g., breakthroughs in food production, metallurgy, state-building, trade, architecture, medicine) and for several if not all of the pasts of the various regions of Africa to be compressed into one, not too creditable and very short (no pre-Arab African civilizations) past that culminated in gunpowder and the slave-trade era, the era which gave birth to ethnic politics.

Cultural traits have been persistently assigned through time to 'natural' ecological zones within a particular culture-area framework. To confuse matters still further, some scholars (e.g., Huffman 1970; Kuper 1980; Huffman 1986) investigating Bantu (or is it non-Iron Age?) history now see ceramic classification and description as the basis of a form of cultural structuralism which seeks universal processes of human mental organization by deliberately becoming ahistorical. Such a perspective is, no doubt, particularly attractive in an intellectual and political climate that wants to see stasis, not change or movement in the past (or even the present) of indigenous African societies (and see Hall 1995).

ETHNOARCHAEOLOGY AND THE RECONSTRUCTION OF CULTURAL VARIETY

Ethnoarchaeology has been proposed as the one effective answer to the problems and questions of archaeological methodology (Atherton 1983:75–104; Agorsah 1990:189–209). Indeed, models applied in the study of cultural activities on sites of various periods, and with behavioural components, have increasingly been drawn from the study of contemporary spatial distributions. Yet, the question remains whether ethnoarchaeology—as presently understood and practised—provides the appropriate breadth and depth for reconstructing the variety and complexity of the cultural realities and contexts of African peoples through a whole range of time periods. Two negative answers are well known: (1) that many of the cultural cases studied by archaeologists concern cultural entities engineered by hominids other than *Homo sapiens sapiens;* and (2) that colonialism, first Arab and later European, so disrupted African cultural identities, as well as relationships which had developed between political and socio-economic groupings occupying the various regions of Africa, that reliance by ethnoarchaeologists on contemporary African societies for their models provides at best a very distorted theoretical framework.

Much current theoretical writing on African ethnoarchaeology runs the risk of being carried away with theorizing about culture and thus neglecting the study of actual peoples. Underlying such excursions into abstract 'cultural' realism is an assumption of objectivity—the assumption that cultural 'facts' can be collected in a systematic and rigorous fashion because there exist 'objective' social and cultural facts. All too often it is taken for granted that 'tribe', 'peasant farmer', 'ethnic group', or whatever, exists for the researcher to 'get at' and use as a data base for comparing one society with another. As pointed out by Cohn (1980:21) this approach to comparison rests on another idea, that of the existence of known

and knowable social/cultural universals, which in turn rests on assumed biological determinants of human culture and society. Such a theoretical emphasis denies a central anthropological discovery, namely that people have always led meaningful lives, and such meanings can only be discovered within the context of those lives.

Generally speaking, neither ethnoarchaeological nor archaeological research in Africa has constructed models derived from the careful study of relevant analogues from *local* ethnographic, linguistic, ecological and historical (especially oral) sources. Only through such a local emphasis can the applicability of data being considered for specific time—and space-bound cultural situations be demonstrated. Such relevant analogues need to be solidly based on, and derived from, the premise that the variety of settlements which peoples own and make use of (when not disturbed by post-depositional movements), contain artefacts produced and/or abandoned as a result of human behaviour, specific to the peoples living in and relating to that place and/or region at that time. Such evidence is the materially visible way that real people brought *their own perception and understanding* of life to bear on the terrain.

DOES AFRICAN ARCHAEOLOGY NEED THEORY?

The search for theory in African archaeology, and indeed historiography in the broad sense, should shed light on the specific philosophies on which certain kinds of investigative stances in archaeology (and indeed in ethnography) are based. The canons for acceptable theory must include *descriptive adequacy* as well as validity. Adequacy presupposes the possibility of differing viewpoints and some measure of truth and correctness. Adequacy and validity thus also involve 'the truth as the native sees it' and a database sufficient to link into such 'truth', in terms of coverage of sites/artefacts identified, and the spread of the sites/artefacts within both regional and temporal boundaries. For African archaeology and material-cultural ethnography to be taken seriously, researchers must aspire to key into the ideological, institutional, structural and operational activities that constitute the life-values of the people being studied. Researchers must produce descriptions that capture the indigenous population's point of view. Thus, their findings must be conveyed in such a way that they are recognizable to the cultural subject, while at the same time making cultural sense to all interested parties.

By its very nature, archaeology seems not to be in a position to satisfy such a canon of 'adequacy' unless certain steps are taken to remedy its inherent weaknesses. The researcher is an outsider from at least the standpoint of time, and often also from that of the culture concerned, and thus has not often captured the subjective experiences of local inhabitants either as individuals or collectivities.

Given that theories of archaeology and ethnography are supposed to enable us to describe different cultures adequately, a vital question has to do with

determining at what point, or points, the quantum leap from describing cultural differences to describing different cultures takes place. How should ethnographers properly *demonstrate* rather than *assume* the existence of different African cultures, as opposed to cultural differences between Africans? What should constitute the significant markers? Which of these remain, and which disappear, with the passage of time? How should archaeologists utilize these for discerning and delineating the existence, make-up, spread and boundaries of different cultures, both historic and prehistoric? Are they correct in taking the very existence of prehistoric, historic and present-day cultures for granted? Would it be possible to suspend the assumption that different cultures exist, so that the implications of that assumption could be examined?

The ethnographer attempts to deduce from what an individual does and says, what s/he thinks an individual is up to. The archaeologist, on the other hand, attempts to deduce from the evidence of things left behind what the individuals actually did, thought, set out to do, and said they did. Culture may be said to be that articulating principle (ideology or ideologies) which a people share, which guides and constitutes its activities—political, economic, social, religious, etc.—and which the observer/researcher is attempting to identify. Viewed from this angle, culture is not a theoretical entity, but rather the theory and practice of the relationships between different kinds and hierarchies of peoples and activities. In this approach there is no room for a monolithic conception of culture which disregards the peculiarities of individual and collective positions. Thus, classical functionalism provides no scope for the researcher to discover what the subject peoples think, or thought, they are, or were, trying to achieve.

If we suspend the *a priori* status of 'culture' and view archaeology and ethnography as enquiries into 'other' cultures, viewed as bodies of discrete shared understanding and knowledge systems, it becomes clear that many situations which, at first sight, appear to be wholesale cultural differences are, in fact, only variations in the way that certain things are carried out by people of the same culture. Ethnographic and archaeological studies need, therefore, to be concerned with discovering the various forms of rationality which peoples have expressed (or manifested) at different times and places under differing conditions, but which are all derived from and based on the same human principles.

Contrary to the picture that has commonly been presented in archaeological, historical and ethnographic studies of African peoples and societies by Europeans, the meeting between European and African cultures is not, and has never been, a meeting between two diametrically opposed worlds. It has never been a meeting between one world, which has exclusive ownership of reason, facts, and objective evaluation, and another world, characterized by emotionality, animal passion, vagueness and irrationality. Rather it has been, and continues to be, a meeting between worlds in which there are alternative viewpoints regarding the reason for living, the objective and subjective aspects of living and life, and evaluation of these from different standpoints. Each has its

own knowledge system, which incorporates clear rules guiding individual and social conduct and action, and a pool of cultural experience, which embodies the actual operation, or rather translation, of this knowledge system into practical conduct and action.

The proper identification of other peoples' real cultures, whether from an archaeological, historical or ethnographic standpoint, can never be the straightforward inductive process which many western scholars studying non-western cultures have represented it to be. The theories of culture put forward by members of that culture never derive from the sort of scientific idealization put forward by many anthropologists; instead, they are *practical* theories used to distinguish, for practical reasons, those who have the 'correct' aspirations, interests or obligations in common. Since they make no claims to invest culture with a distinct ontological status, and the archaeological, historical or present-day ethnographer has no grounds for investing culture with such a status, reseachers should therefore be concerned with viewing and studying other cultures as bodies of discrete shared understandings rather than with *instancing* their distinctiveness.

RECOVERY THEORY

If cultural data are to be recovered properly, strategies and methods need to be devised which do not make assumptions, but rather seek to identify the cultural features and products that are unique and peculiar to the specific peoples being studied, in their specific environment and in their specific time. For the study of African societies, such an approach immediately calls into question the uncritical application of methods and techniques of recovery, analysis and inference, devised for European cultural conditions. One prominent example in archaeology is the grid concept, which is derived from a principle of dividing space into squares and rectangles. Clearly, when African societies built their settlements, farms and roads and regional space using this principle, the grid concept applies; yet, this is far from being the case for those people whose plan concept of settlements and similar structures was/is anything but rectilinear. In such cases, when the grid concept is used, far from being able to decipher a people's spatial forms, structures and patterns, the researcher can only succeed in imposing extraneous ideas. Such imposed ideas lead to the application of unsuitable methods of recovery and analysis. Analyses based on criteria such as the facets of technological (lithics, bones, skin, wood, ceramics, metals) and settlement features usually have little, or no, relevance to African societies' transformation processes.

RECONSTRUCTION THEORY: THE AFRICAN AGENDA FOR BEHAVIOURAL ARCHAEOLOGY

Artefacts and features (including settlements and burials) are the medium through which archaeologists infer the cultural past. According to behavioural archaeologists the irreducible core of archaeology is the effort to ascertain and to explain the relations between human behaviour and material culture in all times and in all places. The general practice is to search for principles of material-cultural dynamics (Binford 1968; Schiffer 1976; Hill 1977) through ethnoarchaeology, comparative ethnography or experimental archaeology. Particular phenomena of the past are usually inferred from the formal, spatial, quantitative and relational properties of artefacts.

In Africa such work has yet to be tackled adequately. In some cases this is because sections or units of sites excavated are often neither particularly appropriate nor representative, either in terms of areal size and/or actual character. In addition, sizes of units or areas excavated are often far from adequate for eliciting the desired (i.e., intrinsic) properties of the entire site, the features and artefacts contained in it. For a long time the practice was to carry out vertical excavations (dig holes) and yet to proceed to make lateral inferences.

There is in this regard an urgent need to study properly the materialcultural histories and ethnographies of African peoples as the basis for finding out and understanding how specific peoples converted a variety of raw materials (rock, bone, clay, skin, wood, metal, etc.) into usable tools, how they used these and with what results (specifically through use- and trace-wear analysis). Instead of uncovering the correlates that link social and material phenomena through solid ethnoarchaeological studies, many workers have merely assumed such correlates and even proceeded to identify societal types on the basis of such supposedly organizational and technological traits (e.g., correlating iron working with Bantu history in Central and Southern Africa). In this respect, on the basis of creatively derived theory from anthropology (e.g., Murdock 1959), some have even gone so far as to link marital residence patterns to the distribution of stylistically defined male and female craft items.

The principal types of cultural formation processes for distinct African settlement types (less so for artefact types) are yet to be identified, because no concerted effort has been made to study African settlement types in their own terms of change within and between contexts of cultural use, deposition, reclamation, disturbance, and so on. Until this happens it will not be possible to elaborate new analytical approaches.

To date, the social theories used to explain variability and change in African human behaviour (at whatever scale or level of abstraction) have usually been drawn from European experience. Archaeologists now need to engage more in the investigation of cultural questions which not only have African peoples as their centrepiece, and their cultural regions and time-spans of occupation as their contexts, but also to identify frameworks and methods which make use of

specific ways to help create and maintain cultural identity and solidarity (Jewsiewicki 1989:10). To accomplish this, researchers must take off any cloak of superiority and meet with Africans not just as equals, but as students. This is the only way to avoid the false belief that western society is the one true society to be an effective model for social science and the development of humans.

THE HIERARCHICAL APPROACH TO INVESTIGATIONS

To achieve the above objectives in the African context would require researchers to discard imposed, arbitrary, alien concepts and methods (principally derived from the English and French), and to learn instead to visualize the regional spread of the cultural set or sub- set in question, and in particular to identify the people's *concept* of their cultural homeland as inferable from relevant oral traditions or other ethnohistorical and/or ethnographic evidence. As pointed out by Langley (1975:98), a social group's perception of its (natural and social) environment, such as it may appear through semantic study, is a *primary* (not sole) avenue for understanding the relationship between its social structures and its organization of space. Yet practitioners of orthodox African archaeology have rarely been interested in trying to decipher, from either the oral lore or ethnography of African societies, what these may reveal regarding the methods whereby groups elaborate and translate knowledge. Such concepts, explanations and arguments must always have been in use in the daily lives and traditions of all Africans, from earliest times to more recent historical periods characterized by rural farming, a pastoral way of life or urban development.

Concept-building and related forms of modelling and abstraction helped, and continue to help, Africans—as indeed other peoples—to structure their understanding of their environments and their interactions with them (Langley 1975). Although the slave trade and colonialism certainly had an adverse impact on African identities and worlds, ending up by distorting, interrupting and restraining, they were never able to erode the core features of African characteristics and identity. The continued imposition of European language concepts such as band, ethnic group and village, only continues to prevent researchers from getting to the roots and nuances of the social and cultural concepts of African peoples, their popular science traditions and, therefore, the construction of appropriate re-enactments of their cultural histories.

Ethnographic data approached in the right manner enable one to understand the ecological basis and character of certain concepts in many African cultures, especially as regards (1) their dominant modes of production; (2) the precise or specific ways that African peoples define spatial and temporal categories in their languages; and (3) in what respects the structures of their language relate to the natural space inhabited by the society, as well as the use made of such space (and see Langley 1975).

Only by keying into the conceptualizations of African societies, both rural and urban, through ethnographic and oral historical sources, can the researcher gain entry into the fabric of African knowledge systems, not just for the present-day cultural landscapes and material-cultural categories/systems, but also for other discernible and distinct and sometimes quite different cultural landscapes of earlier periods (e.g., those before the slave trade). Such knowledge provides a valid base for identifying the social and environmental plans and designs of past populations, and for formulating action in social and environmental planning today.

Such ethno-linguistic studies, whether of basic geographical or environmental terms, or technological, economic and socio-economic ones, only come into their own when, as Richards (1975:106) observes, 'We begin to consider complete terminologies, when for example we look not at isolated words for rocks, soils and plants (vegetation), animal and cultural facts and artefacts derived therefrom, but at whole sets of terms and complete taxonomies covering the biological, social and cultural domains.' A study, for example, of the principles governing the naming and classification of elements within the environment (natural, social and temporal), makes it possible to begin to understand how local populations of hunters, farmers, pastoralists or metal-workers, conceptualize the important facets of their world (e.g., settlements, societal units, politics, beliefs, food, clothing, ornaments, temporal units, etc.) and proceed to bring these into effect through verbal and/or non-verbal operations, and the ways they interrelate these worlds to constitute their one world. Getting to know the people's classificatory principles ought also to provide insight into the fundamental process of linguistic and symbolic coding which permits a society either to replicate or to change its social structure and culture from generation to generation. In many ways, the use of oral history—in the contexts of both myth and genealogies—is more problematic. This is particularly so given the fact that, with the advent of Islamic and, later on, European and related influences, many peoples in the African societies affected cultivated the habit of constructing legendary and socially prestigious genealogies for themselves, often deriving them from somewhere in the Near East or the northern parts of Africa, or wherever this socially prestigious influence was seen to come from.

CONCLUSION

For African archaeology to be liberated from the shackles of colonialism it has to become an historical science that distances itself from the present discipline, which studies illusory entities and reduces human beings to mere chessboard pieces, as if they were part of an organic world totally under the control of the physical and mathematical laws of Science and Nature. It has to be an archaeology firmly founded on the fact that historical, and not mathematical or any other scientific awareness is the only form of self-knowledge; that history is also open to constant reinterpretation in what seems to be a universal quest for

useful precedents (Ajayi & Ikara 1985:6); that history is an anvil of identity which is vulnerable to distortion by any who aspire to a monopoly of power. It has to be an archaeology which recognizes and actively seeks to counter the fact that tyrants, whether colonialists or their successors 'are terrified at the sound of the wheels of history…so they try to rewrite history, make up official history; put cotton wool in their ears and in those of the population. Maybe they and the people will not hear the real lessons of history' (Ngugi Wa Thiongo 1987:XII), lessons which teach of struggle and change. What is needed is a socio-historical archaeological discipline which not only recognizes that much of Africa's written and unwritten history has sided with its rulers rather than with that of its people (Lonsdale 1989), but also proceeds unambiguously to correct and offset this imbalance.

The historical past of Africans covers the whole span of human past activity, from its origins to the present. It was neither single nor simple, but rather multiple and complex like the functions of human memory. To discover and represent this multiple and complex historical Africa adequately often requires anthropological, historical and archaeological presentation of a drastically different kind from that which is currently regarded as standard in these disciplines today.

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CHAPTER SIX
THEORETICAL PERSPECTIVES IN INDIAN ARCHAEOLOGY
An historical review

K.PADDAYYA

Researchers in Asia, Africa and other parts of the world that experienced prolonged periods of colonial rule by European powers may adopt divergent intellectual positions regarding the nature and extent of the contribution made by the colonizing powers to the study of the historical past and cultural heritage of their respective colonies. Perceptions of this contribution will have to take into account a variety of factors such as the nature of the colonialism practised in a given region, the ultimate motives and interests the colonial rulers had in mind while initiating studies of the region's past, the world-view of those who actually conducted the studies and, of course, the time-depth and character of the past available for study. Moreover, evaluations of the role played by Europeans, far from remaining static, will undergo periodic revisions resulting from changes experienced in academic perspectives in general, and even changes in the world order from time to time.

It therefore surprises no one to see now and then researchers from the erstwhile colonies rising to their feet and, far from complimenting their European predecessors on their efforts, treating the whole enterprise as a part of the story of exploitative colonialism; as an expression of the European world-view, which dismisses the rest of the globe as the inferior *Other* and its inhabitants as barbaric and indolent (e.g., see Andah 1995). Said's book *Orientalism* (1978) is a well-known example of this hyper-critical attitude, with special reference to Europe's research into the past of the Arab East. Said is influenced by the writings of Foucault and treats Orientalism as a discourse. In his own words:

> Taking the late 18th century as a very roughly defined starting point Orientalism can be discussed and analyzed as the corporate institution for dealing with the Orient—dealing with it by making statements about it, ruling over it: in short, Orientalism as a Western style for dominating, restructuring and having authority over the Orient.
>
> (Said 1978:3)

Said argues that it is by defining the 'Orient' as the inferior civilizational *Other* that Europe defined itself.

This intellectual trend—variously known as post-orientalism, orientalism-in-reverse or, as Ahmad mockingly (1991; 1993:159–219) calls it, indigenism—is fast gaining ground. As far as India is concerned, Inden's book *Imagining India* (1990; see also Inden 1986; Cohn 1992) is probably the most comprehensive attempt made thus far to carry forward the trend set by Said. Drawing upon Indological writings, as well as writings bearing upon disciplines such as the history of religions, anthropology, economic and political philosophy, produced by western scholars since the Enlightenment and using as his tools ideas adopted from the works of philosophers of history like Collingwood and those of the post-structuralists like Foucault and Gramsci, Inden attempts to lay bare the presuppositions and contradictions inherent in writings on ancient India. His main argument is that the determinist scientific approaches adopted by western scholars led them to perceive India as consisting of simple, unchanging essences. This essentialist tradition has denied India the element of human agency—the capacity of its people to order their world. Inden takes upon himself the task of rediscovering the role of this element of human agency in ancient India with reference to institutions like the caste system, kinship organization, etc. Earlier some Indian writers (e.g., Sharma 1961; Sharma 1966; Thapar 1984a: see also Thapar 1992:1–22) had drawn attention, albeit in a milder tone, to the distortions introduced by European writings on ancient India.

The present chapter, devoted to an historically oriented review of the theoretical perspectives in Indian archaeology, is by no means an addition to this hyper-critical attitude being developed towards European scholarship about the ancient Orient. The reason is that the motives of the contemporary anti-western critical enterprise are not always genuine; this enterprise 'resembles a stock exchange with several brokers trying to outshout and out-bid one another in an attempt to corner the market' (Paranjpe 1990:159). This chapter adopts a more positive attitude; its main plea is that total debunking of Europe's contribution to the understanding of India's past would amount to throwing out the baby with the bathwater. To make the point in a more polite way, it is necessary to avoid what Eco (1992) thoughtfully called over-interpretation, while giving vent to his distress at the excesses committed in literary criticism in the name of deconstruction. It is quite probable that the work of European scholars was motivated by, or had the unintended effect of, furthering missionary activity, territorial expansion and economic exploitation. Yet these considerations need not make one blind to the feelings of reverence and respect, and even of affinity, with which some of the European researchers approached India's past.

In this respect I find much sense in the attitude commended by Ingalls (1960: 197):

> The motives for studying the past seem to me as various as motives of humans for any other endeavour…. Let us be thankful for man's infinite variety and instead of arguing about the approaches and motives of

> scholarship concentrate our criticism on the results. On the results, I think, we will find ourselves essentially in agreement.

It is this attitude which I adopt in this chapter. My main point of emphasis is that archaeology in India is a European innovation. As is the case with archaeology in most European countries (see Hodder 1991), it has no doubt had its own developmental trajectory. This is particularly true in respect to the nature and extent of the impact of developments in theory that has made itself felt in the Anglo-American world during the last thirty years. Further, I believe that Indian archaeology has potential for employing indigenous epistemological traditions to study its past. Likewise, the role that the study of the past can play in the present cannot be divorced from the existing socio-political milieu in the land.

INDIGENOUS TRADITIONS FOR STUDYING THE PAST

Until the early decades of this century it was almost routinely held by European orientalists that, unlike the ancient Greeks and the Chinese, the Indians lacked a sense of history; in fact, they were branded as an ahistorical people. Pargiter (1922) was the first to attempt a comprehensive critical review of this point of view. He made extensive use of the *vamsavalis,* or genealogies, provided in the *Puranas* for reconstructing the political history of the earliest phase of ancient India. Since then a number of researchers (e.g., Warder 1959; Pathak 1966; Warder 1970; Thapar 1984a; Thapar 1992) have undertaken a detailed examination of the historiographical value of ancient Indian writings (mainly Hindu, but also Buddhist and Jain sources); unfortunately, however, the results of their work are not widely known among archaeologists.[1]

The main conclusion emanating from such recent work is that while the Indian historical tradition may be lacking in critical scrutiny of events and no rational arguments may have been advanced for judging causality, ancient India did leave behind materials and information of much historical importance and interest. The whole argument has been summed up well by Thapar: defining a sense of history 'as a consciousness of past events, which events are relevant to a particular society, seen in a chronological framework and expressed in a form which meets the needs of the society', she concludes that ancient Indian society was eminently historical (Thapar 1984a:269). She further claims that, in tune with the lineage-based character of early Indian society, the historical consciousness (or what she calls the *itihasa purana* tradition) was one of the embedded type. With the development of a full-fledged state-level society during the first millennium AD a more definite form of historical writing developed. Thapar calls it externalized consciousness of history.

The Hindu concept of time differs from that found in Judaeo-Christian thought in two respects (for a general survey of the Hindu, Jain and Buddhist cosmologies, see Kloetzli 1987; Basham 1991:322–3). As against the 6,000–odd

years of existence allowed for the world by Christian theology, the ancient Hindu traditions envisaged immense duration for time; the *Bhagvadgita* regards time *(kala)* as imperishable. Central to the Hindu tradition is the concept of *mahayuga* or the Great Cycle lasting for 4,320,000 years. It is divided into four units or *yugas,* respectively known as the *Krita, Treta, Dvapara* and *Kali*. We are currently in the *Kaliyuga,* which began in 3102 BC and has a duration of 432,000 years. It will end in destruction by flood and fire. This destruction will be followed by a new cycle or *mahayuga* and so on. An idea of the vastness of time provided for by Hindu cosmogonomy can be deduced from the life-span allowed for Brahma, the creator god. *Kalpa* or a day of Brahma has 4,300 million years; his night is of equal duration. Three hundred and sixty days and nights of this magnitude form a year for Brahma. He has a life of a hundred such years and is now said to be in his fifty-first year. When Brahma completes his life-span (a staggering figure of 311,040,000 million years), the universe returns to its ineffable world-spirit and then another creator god appears on the scene. In this connection it is interesting to note that the time duration provided for one day and one night of Brahma–8.6 billion years—falls within the limits of the age-estimate (6.5 to 13 billion years) provided for the universe by modern astronomy.

Closely linked with this concept of the vastness of time is the notion of its cyclical nature, which contrasts with the western idea of linear movement and the associated notion of the progression of historical events. Underlying the Hindu division of time into *yugas* is the notion that there was a golden age in the beginning when the social order was working smoothly and the king was a benevolent personality. Thereupon a slow process of degeneration set in, implying retrograde movement, finally culminating in the total destruction of the world. After this a new cycle of prosperity commences. Some writers have felt that the elements of destruction and retrogressive movement found in the Hindu concept of time inhibited development of the notion of purpose in history. Thapar, however, disagrees with this view and points out that 'the Hindu cycle concept of time is essentially cosmological in character and did not prevent recording of the past in a form considered socially relevant and necessary to the present and the future. Such a cyclic concept emphasized continual change…it was maintained that the past can and does teach lessons, usually moral lessons…' (Thapar 1984a:287).

The Buddhist tradition also provides for an incalculable duration of time; its movement is said to be in the form of waves. This wave notion once again postulates a Golden Age in the initial stages and as man becomes more and more acquisitive, degeneration sets in and the age ends in evil and strife. Then there is a revival and return to the Utopian age, marking another wave.

The earliest known actual materials consist of five forms of oral compositions: viz *gathas, narasamsis, akhyanas, itihasa* and *purana* (Pathak 1966:3–8). *Gathas* are songs celebrating the heroic deeds of rulers, sages and even gods. *Narasamsis* are praises celebrating men, including, at first, deceased fathers and

things and ideas associated with them. *Akhyanas* are historical narratives giving accounts of events of various kinds including wars between gods and demons. These narratives served as a source for themes portrayed at a later date in historical dramas and the two epics (*Mahabharata* and *Ramayana*). *Itihasa* (meaning ancient events or, literally 'verily thus it happened') covers historical compositions. *Purana* is the fifth form and means ancient lore.

All these compositions were basically part of the sacred literature. The antiquity of *gathas* and *narasamsis* goes back to the Rigvedic times. All these compositions were used for purposes of daily recitation and were also recited on important occasions such as weddings and horse-sacrifices. Until the fourth century BC these traditions remained at the level of a loose collection of legends and experiences. They were characterized by fluidity and constant revision. As part of a regrouping, some of the smaller events taken from *gathas* and *narasamsis* were welded together, while others were inserted into *itihasa* and *purana*. The latter two forms are closely interlinked, and their affinity is highlighted by the creation of the blended form or compound *itihasa-purana*. This eventually emerged as the dominant tradition. The two epics *Mahabharata* and *Ramayana* belong to this tradition.

Thapar (1992:137–73) believes that the *itihasa-purana* tradition, with its substratum made up of *gathas, akhyanas* and *narasamsis,* had taken firm root by the fourth century BC and subsequently passed through two or three stages of growth. These stages reflect changes in the form of expression of historical consciousness, which in turn represent changes not only in political forms but in the totality of society.

During the first stage (from the fourth century BC to the fourth century AD) the task of collecting information and presenting it in literary form was the special task assigned to the *sutas* and *magadhas* (bards and chroniclers), who were the descendants of priestly families of the Vedic period. The historical tradition basically consisted of genealogies or *vamsavalis* of rulers and kings. Thapar (1984a:326–360; 1991) argues that they were required for authenticating the legal rights and social status of various rival units of the Aryan population that had sprung up all over northern India by this time. This was the stage of embedded historical consciousness, and the lineage-based societies profited by using historical information pertaining to the ordering of lineages.

From the fourth century AD (the time of the establishment of the Gupta empire) the compositions of the *itihasa* tradition, as represented by the *puranas,* began to evolve from their oral form in Prakrit into Sanskrit, in which they were written down. This period (fourth-sixth centuries AD) was one of momentous changes in the society of northern India. The lineage-based social structure gave way to state supremacy based on dynastic power. The *Brahmanas* took over from the *sutas* and *magadhas* the job of keeping the genealogical record. The new rulers had to be legitimized by supplying links with the gods and kings of antiquity.

The historical record available in the *puranas* forms part of a wider worldview (for details see Pargiter 1922). It is the *vamsanucharita* portion of the *puranas*

which contains historical information about origin myths, the chief among which is that which describes the Great Flood and narrates how Manu, primeval man, was saved by the god Vishnu taking the form of a fish and from him future mankind was born (Thapar 1984a:294–325). The second section provides information about various dynasties; names of rulers and the length of their reigns (Pargiter counted 95 generations from Manu to the Mahabharata war); geographical distribution of dynasties/tribes and their socio-political formations; and the relative importance of agricultural/pastoral practices in the economy. Thus the *puranas* constitute an invaluable source of historical information, although one must remember that until the sixth/seventh centuries AD this tradition was still a part of religious literature.

Writings belonging to the historical tradition as a form of secular literature emerge in the next phase dating between the seventh and twelfth centuries AD. We thus come to a class of literature or texts known as the *charitas* or royal biographies. These are historical narratives written down by court poets to record the major achievements and qualities of their respective patrons. These narratives were probably inspired by the eulogistic inscriptions or *prasatis* of earlier periods such as the Hathigumpha inscription of the Kalinga ruler Kharavela and the Junagadh record of Rudradaman. The writing of *charitas* continued beyond the twelfth century. Thapar (1984a:274) attributes the rise to prominence of these historical biographies to a fundamental change in the structure of political power —the shift of interest from tribe to king and his court, the rise of small regional kingdoms and consequent growth of local loyalties. The *suta* now faded into the background and his place was taken over by the court-poet.

Finally, there are the texts known as *vamasavalis* or regional chronicles. These were also written down by court-poets who utilized the various local *puranas* as well as oral traditions for this purpose. These chronicles became the typical literature of regional kingdoms that had emerged in different parts of the country during the period covering the late first and early second millennia AD. These usually start off with origin myths concerning the region and the dynasty, then endeavour to link up local history with origin myths and genealogies of the Great Tradition (as constituted by the *puranas*) and finally provide detailed accounts of the history and contemporary events of the respective ruling dynasties. Regional chronicles of this type written in local languages are known from various parts of the country from the extreme north to Kerala in the south.

Perhaps the best and most widely known example of the *vamsavalis* is provided by Kalhana's *Rajatarangini*. This text is dated to the twelfth century AD and deals with the history of Kashmir from the earliest times until the twelfth century. Kalhana considered himself to be a poet *(kavi)* and relied on sources of various kinds for preparing this chronicle: previous genealogies, inscriptions, coins, even visits to historical sites like monasteries and temples, the use of written and oral sources and, finally, on his own memory (Stein 1961:24–7).

Most of the writers consider *Rajatarangini* to be the first genuine attempt at history writing in India (and see Basham 1961). Kalhana commented upon both

the method of writing history and the purpose of history. According to him, an historian should critically examine the writings of previous historians. He must keep a detached mind; rather like a judge, he must avoid both bias *(dvesha)* and prejudice *(raga)* while recounting events. The historian's task is to make 'vivid before one's eyes pictures of a bygone age'. Over and above the task of discovering the truth about the past, history, according to Kalhana, has moral lessons for the present. In his opinion, by studying the history of earlier periods, the wise might forsee the future. 'This saga which is properly made should be useful for [a] king as a stimulant or as a sedative, like a psychic, according to time and place' (Majumdar 1961:21). It is remarkable that Kalhana was already anticipating the present-day debates stressing the need to place the study of the past within a contemporary, socio-political context.

It may be noted that causality was not given free rein in the Hindu historical explanation of history. *Dharma* and *Karma* were seen as the main forces shaping the flow of historical events. *Dharma* is the socio-religious ordering of society; all events were generally seen as following the laws of *Dharma*. *Karma* (past deeds being the explanation of the present condition) provided continuity between past and present. These two concepts together thus introduced a definite element of determinism into the Hindu treatment of historical events (Thapar 1984a:288).

Once the country came under the rule of the Turks and Arabs, the ancient Hindu historical tradition of writing biographical accounts and regional chronicles started to fade away. During the twelfth and thirteenth centuries AD, however, the Jain teachers of western India wrote compositions of two types, both containing historical information. These are *prabandhas* or narratives relating to the royalty and genealogical chronicles concerning local kings and states.

As regards the Buddhist historical tradition, the Pali chronicle of Sri Lanka constitutes the earliest known source (Perera 1961; Warder 1961; see also Gokhale 1979). *Dipavamsa* and *Mahavamsa* are the major texts. Like the Hindu writings these also began as oral traditions and were written down only during the fifth-sixth centuries AD. The *Dipavamsa* narrates the history of the island of Sri Lanka. The *Mahavamsa* is the chronicle telling of the history of the Mahavihara at Anuradhapura. It is based on various sources such as royal records, monastic documents, histories of relics and shrines, legends and folklore.

The second category of evidence bearing upon the indigenous tradition for studying the past consists of references to the prehistoric way of life as depicted in some of the ancient Indian texts. The Jain text *Kalpasutra* of Bhadrabahu described the condition of humanity at the time of Rishabha, the first Jina or Tirthankara. Rishabha was the son of King Nabhi. Prior to Nabhi's reign there were *Kalpavrikshas* or wish-fulfilling trees which granted people whatever they wished. Because these trees disappeared in Nabhi's reign, bad times befell people who did not know how to till land and cultivate grain; starvation set in. So Nabhi taught people how to make pots out of the temples of elephants, to crush

grain with a pestle, to make fire, and to cook. He further explained to them the method for drawing thread out of cotton and weaving cloth. Sankalia (1957:82–3) believes that these literary references probably preserve the memory of the onset of the settled agricultural (neolithic) way of life, following upon a long hunter-gatherer stage.

Another reference to prehistoric times or change from a nomadic to a settled way of life is provided in *Samaranganasutradhara,* a Sanskrit text on architecture written by the central Indian king, Bhoja, who ruled in the eleventh century AD (Pandey 1989). It is mentioned in this text that in the *Kritayuga* men were dwelling in groves, hills and forests, and near rivers and lakes, along with gods. The wishing-tree *Kalpavriksha* catered for all kinds of needs. It was subsequently lost, so the people were forced to make use of tree-foods. Later on they reaped the grains of wild rice. They also employed stone tools (polished axes?) for cutting down trees. The wood thus obtained was used for constructing houses ranging in size from one-room dwellings to seven-room structures.

Another instance of a postulated relation between textual references and a prehistoric way of life is provided by Prasad's (1989) correlation between puranic evidence and archaeological data (including the palynological record from the Rajasthan lakes) regarding the beginning of the neolithic or agricultural way of life. He adopts shortened durations for the four *yugas*. According to him, the seventh *Manvantara* (in which we find ourselves at present) started in 8530 BC and this he equates with the beginning of the Holocene. Citing the evidence of the *Vayupurana,* he believes that in the *Kritayuga* (8508–7548 BC) humans led a nomadic way of life without houses, agriculture or any noteworthy socio-economic activities. Prasad correlates this way of life with the mesolithic phase. *Tretayuga* set in by 7300 BC and people started collecting seeds of food-grains. This marks the onset of the neolithic phase, as represented at sites like Mehrgarh in Baluchistan.

The third category of evidence pertains to measures adopted towards the conservation of cultural heritage in pre-modern India. Nagaraja Rao (pers. comm.) has collected a body of data from both textual sources and epigraphical records which shows that structural conservation of monuments and repairs to mutilated sculptures were being undertaken in India since early times and continued till the nineteenth century (e.g., the famous Vijayanagara ruler Krishnadevaraya (sixteenth century) gave grants for the restoration of temples in the territory conquered by him in southern India; likewise, the palace-temple of Mysore was restored by the ruling Wodeyars in the seventeenth century). In this context we should also recall the antiquarian interest shown by Kalhana in the coins, epigraphical records and ancient sites of Kashmir, as well as other instances such as the shifting to Delhi of the Asokan pillars from Meerut and Ambala by the later Muslim ruler Feroze Shah Tughlak.

Finally, it is also relevant to note the availability of a number of Indian texts (in Sanskrit or vernacular languages) on architecture (Vastusastra). Ever since the publication of Ram Raz's book *Essay on the Architecture of the Hindus* in

1834 [1972], several attempts have been made by Stella Kramrisch and other scholars to relate the temples and their styles and construction methods to the principles embodied in the ancient texts. In the first quarter of this century important texts were brought to light dating from the eighth century to the thirteenth century AD. These texts (Sompura 1975) were conceptual aids to the raising of temples and were in turn revised and refined in the light of experience gleaned from the actual constructions. As such, these constitute a synthesis of the ancient Indian understanding of temple styles, their structural components and symbolic meanings (for details, see Chandra 1975).

So far attention has been focused on the four major categories of evidence preserving clues to the interest evinced by Indians in their past. The crucial question now is: do these really justify inferring the existence of an indigenous tradition for studying the material remains from the past? At present it is difficult to reply in the affirmative, for some sources deal exclusively with textual data: these, while no doubt illustrating and authenticating the antiquity of historical tradition in India, do not have a bearing on the study of archaeological remains. The third and fourth strands of evidence do indeed concern archaeological remains but the aim appears to be either to attend to repairs of structures and objects or sculptures in connection with the practical maintenance, or to offer guidance to temple construction activity, rather than to gain knowledge about the past; hence these cannot be said to constitute a regular tradition for the study of antiquarian remains. Thus their relevance to the historiography of Indian archaeology at the moment must remain doubtful, or at least marginal.

EUROPEAN INFLUENCE

The rediscovery of India's past was a European effort. Britain claims a major share in it and is followed by Germany, France and other countries. In the early phase stretching from the sixteenth century through the eighteenth century European interest in Indian antiquarian remains consisted of personal curiosity on the part of travellers and sailors who visited sites like the Elephant caves, rock-cut temples at Mahabalipuram and the temples of Orissa situated on the western and eastern seaboards.[2] Maurice's (1812–14) seven-volume work was the first comprehensive account of the antiquarian remains of the country. He considered the Indian temples alongside those of Greece, Egypt and Mexico. The founding of the Asiatic Society of Bengal at Calcutta in 1784, led by the learned judge Sir William Jones, was the first step to place this pursuit on an organized footing (Fig. 6.1). The aim of the Society was to inquire 'into the History…the Antiquities, Arts, Sciences and Literatures of Asia' (see Kejariwal 1988). Apart from being a manifestation of the urge to know the land and its people better in order to be able to rule over them more effectively, this event simultaneously reflected the influence exerted by the rise of antiquarian societies in Britain.

The Society also symbolized the desire of the West to free itself from the hold of Judaeo-Christian thought. It is this attitude, as fostered by the Age of

Enlightenment, that led some European thinkers to regard India as the original home of civilization. Voltaire, for instance, dared to say in a private letter to M.Bailly that 'Everything has come to us from the banks of the Ganges—astronomy, astrology, metempsychosis, etc.' (as quoted in Poliakov 1974:185). Standing on the deck of his ship in August 1783 and discerning the shores of India on the horizon, the young William Jones was deeply inspired and visualized the land as 'the nurse of sciences, the inventress of delightful and useful arts, the scene of glorious actions, fertile in the production of human genius and infinitely diversified in the forms of religion and government, in the laws, manners, customs and languages, as well as in the features and complexions of men' (see Cannon 1985; Cannon 1990). He felt sorry that such a land as this had not been investigated at all. Sir William viewed India's past as a part of Universal History. His recognition of the affinities between Sanskrit, ancient Iranian and European languages such as Greek and Latin laid the foundation for IndoEuropean studies. Some of the other writers of this period were equally enchanted by the ancient culture of India and went to the length of recognizing Buddhist influences as far afield as Scotland (Wise 1857, as quoted in Chakrabarti 1976). For reasons not difficult to grasp this trend of thought was reversed after the Sepoy Mutiny of 1857; India was now placed at the receiving end of a continuous flow of cultures and peoples from outside.

The early part of the nineteenth century witnessed some brilliant achievements in Indology. Of particular importance were contributions to Sanskrit studies, the decipherment of ancient scripts, ethnographic survey of the region from Madras to Malabar and later in Bihar, the discovery of the Amaravati stupa (Figs. 6.2–3), explorations in the Punjab and Cunningham's (1871) work at Sarnath and Banaras. However, with only rare exceptions, the researchers concerned took few measurements and provided no plans. Cunningham later rightly called these early workers 'closet or scholastic archaeologists'.

Archaeological research in a regular sense began to take shape in India from about the middle of the last century. Meadows Taylor (Figs. 6.4–6.5) (1865, 1927, 1941), James Fergusson (1845, 1876, 1974), Alexander Cunningham (1871) and Robert Bruce Foote (1916) are undoubtedly among the major figures. Foundations were laid by them for what we now call prehistory, proto-history, art history and historical archaeology. Viewed in a wider context, these developments were a reflection of the important changes that were taking place in what Stocking (1987:Chs 6, 7) has termed the Victorian anthropology of the mid-nineteenth century in England. A few details reveal the nature of those leading the spirit of enquiry.

Taylor spent his entire administrative career (1824–58) in the service of the Hyderabad State. He was a true polymath of the Victorian era. Even without having the advantages of school education, Taylor achieved distinction as an engineer, scientist, ethnologist, historian, archaeologist and novelist. His is the classic case of a person overcome by complete culture shock on landing in India: he wrote to his mother back in England that 'the servants here are the laziest lot

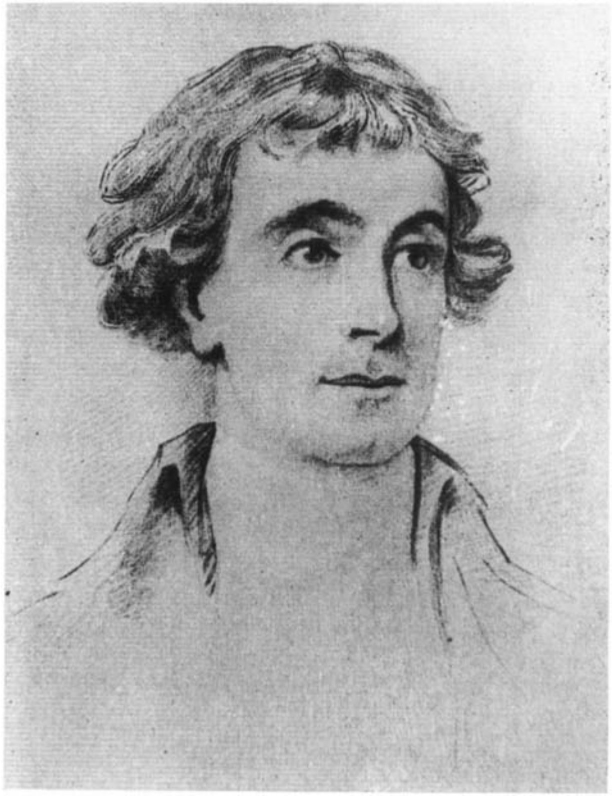


Figure 6.1 Sir William Jones (1746–1794), Calcutta Supreme Court judge who founded the Asiatic Society of Bengal in 1784 (After S.Roy, 1961, *The Story of Indian Archaeology 1784–1947,* New Delhi: Archaeological Survey of India)

of rascals under the sun…they dress up in the most ridiculous manner, carry torches in their hands and go on with all kinds of antics…the black fellows are such queer 'jummies' with large bracelets on their arms and thighs made of silver and rings through their noses and strings of beads round their necks and almost naked…' (Taylor 1927:18–21). Yet he soon embarked upon a process of learning to understand the country and its people and their past that finally made him take a U-turn and urge the British to visit India where

> They would see an intelligent, industrious and, in spite of incomprehensible idolatry and superstition, an amicable people. They would see good husbandry and a fertile country, and they would return with a conviction that the Mahrathas and the Mahomedans, who live there are a reasonably civilized people, not painting their faces, carrying tomahawks, marching on war trails and dancing war or peace dances, according to the customs of North American Indians; but that they are the

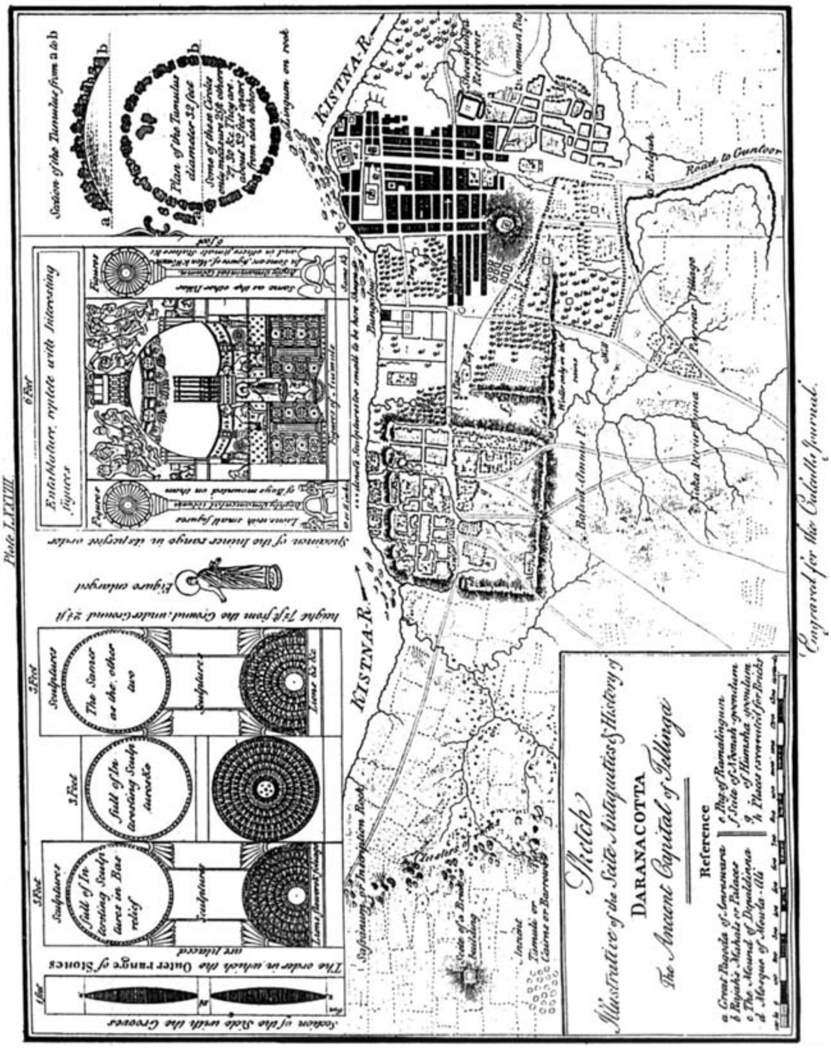


Figure 6.2 An 1817 map of Amaravati prepared by Colonel Colin Mackenzie, showing the modern town, Buddhist stupa, the early historic habitation area and a complex of Iron Age stone circle graves (After Colonel Colin Mackenzie, 1822, in *Calcutta Journal* vol. iv, no. 179, 27 July 1822)

> descendants of men who even before the ages of European antiquity had executed works of masterly skill, and who professed a religion which has exercised prodigious influence over vast numbers of mankind.
>
> (Taylor 1865:23)

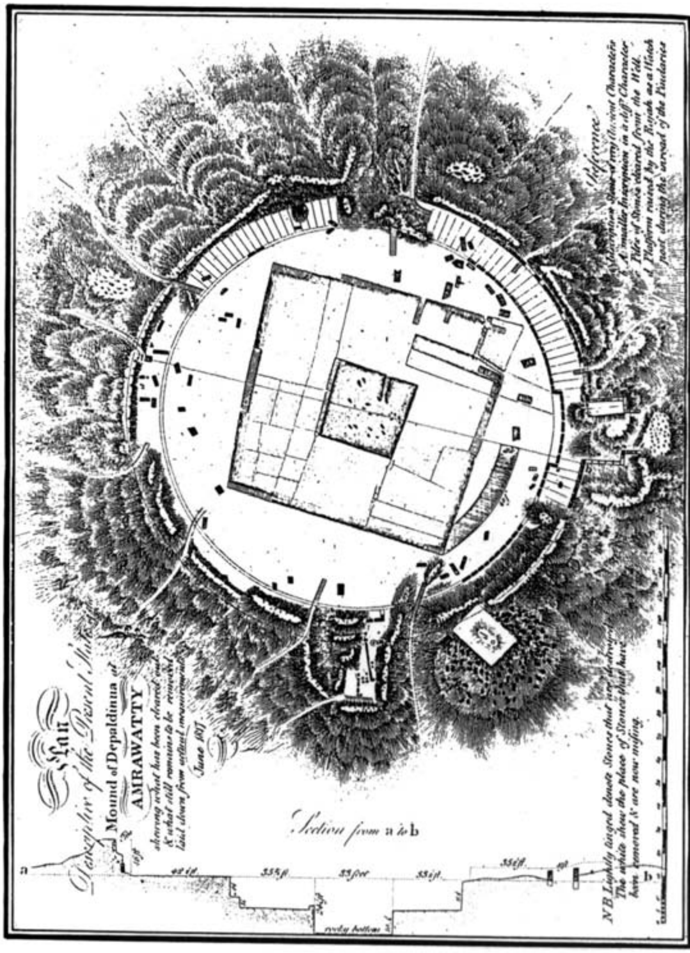


Figure 6.3 Colin Mackenzie's map of the Buddhist stupa at Amaravati (After Colonel Colin MacKenzie, 1822, in *Calcutta Journal,* vol. iv, no, 179, 27 July 1822)

During his tenure (1842–53) as the Political Agent of the British in the principality of Shorapur in North Karnataka, Taylor discovered a number of sites containing Iron Age graves comprising dolmens, cists and circles. It is most probable that these discoveries were inspired by the studies on similar sites in the Carnac region of Brittany and England made by Logan and other British antiquarians in the first half of the last century (Sherratt 1987). In fact Taylor already recognized the broad similarities between the Indian and western European megalithic types and believed that the Indian sites owed their existence to the entry from outside of Druids or Druidic Scythians (Chakrabarti 1976).

Taylor published several papers on these discoveries giving accurate and detailed information (Figs. 6.6–7) (Taylor 1941). The preparation of accurate site



Figure 6.4 Colonel Meadows Taylor (1808–1876) (After Meadows Taylor, 1920, *The Story of my Life,* new edition with introduction by Henry Bruce, Oxford: Oxford University Press)

maps, excavation of burials under personal supervision, use of the principle of stratification for recording soil sediments encountered in the excavation (Fig. 6.7), preparation of drawings of various categories of objects including human skeletal material and even the discovery of habitation sites all contribute to him being recognized as a field archaeologist of an exceptional kind. This was at a time when there was hardly any field archaeology worthy of the name anywhere in the world. We have necessarily to presume that Taylor adopted these methods as a matter of common sense. He was obviously far ahead of his time: it was only in the latter part of the century that techniques of modern field archaeology began to be developed in England by workers such as General Pitt-Rivers. Taylor's achievement thus 'stands out as a landmark in the annals of archaeology' (Wheeler 1961:23).

Likewise, James Fergusson, a Scottish businessman and indigo-planter, laid the foundations for a scientific study of ancient Indian architecture (see Chandra 1975, Chandra 1983 and Mitter 1977 for subsequent studies of temple

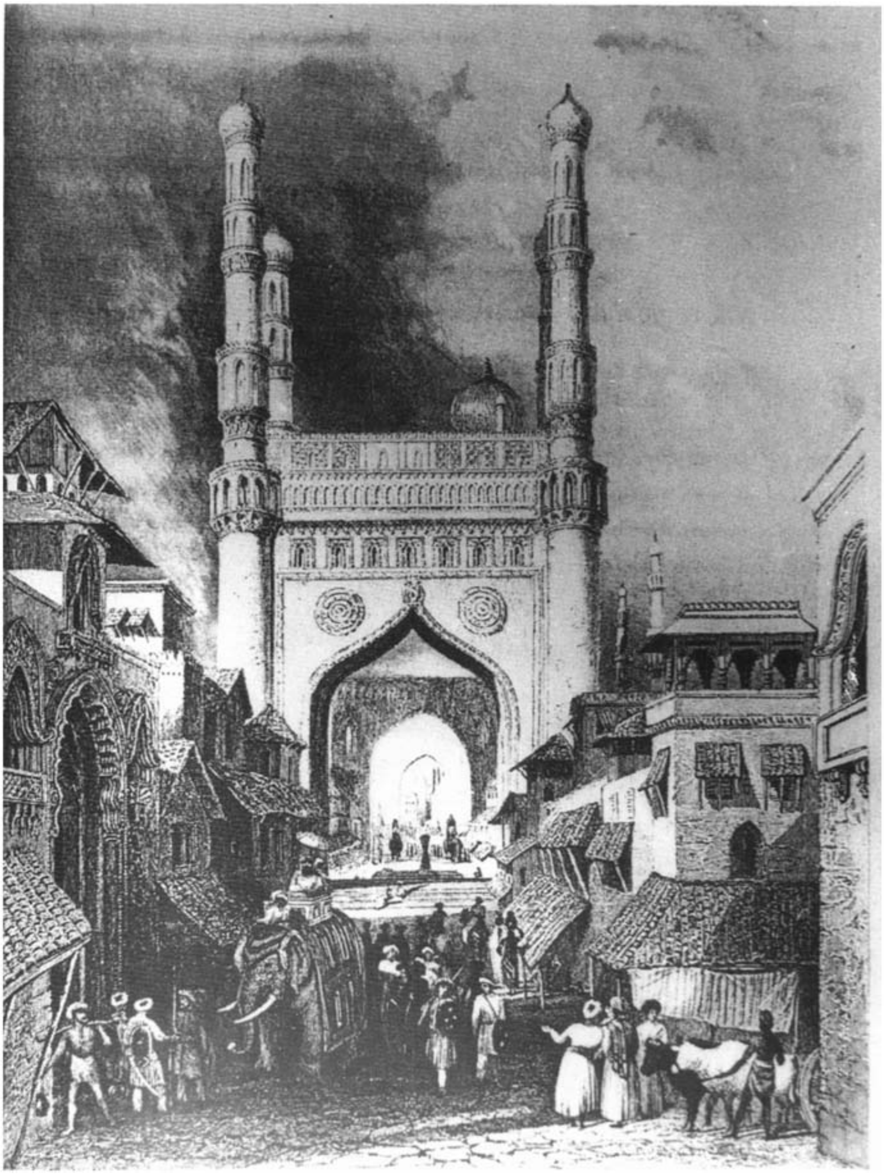


Figure 6.5 Meadows Taylor's sketch of Char Minar ('Four Towers') in Hyderabad, built in 1592 by the Qutb Shahi rulers (After Meadows Taylor, 1837, *Sketches in the Deccan,* London: Charles Tilt)

architecture, sculpture and painting). The decipherment of ancient Indian scripts inspired Fergusson to take an interest in the antiquities of India, a country which he characterized as 'a great and most poetic region of the globe'. In spite of his many prejudices including a belief in European racial superiority, he claimed to

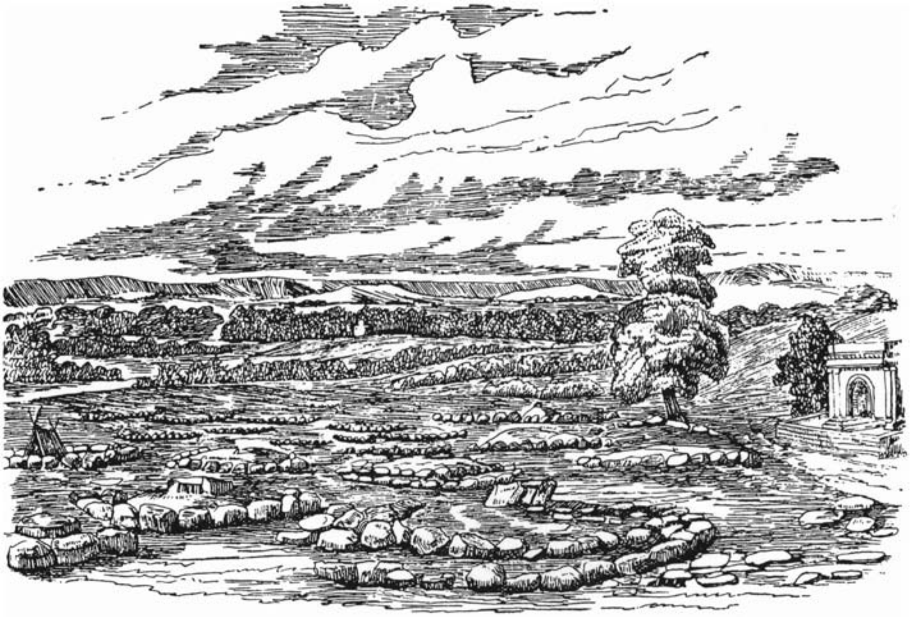


Figure 6.6 Meadows Taylor's 1853 sketch map of stone circle graves at Jewargi (Jiwargi) in the Deccan (After Meadows Taylor, 1941, *Megalithic Tombs and Other Ancient Remains in the Deccan,* Hyderabad: Hyderabad Archaeological Department)

be a 'philosophical student' of ancient Indian architecture, about which at that time, in his own words, there was only 'darkness and uncertainty. There is scarcely a work on architecture published, or a lecture read, which does not commence by a comparison between the styles of India and Egypt' (as quoted by Chandra 1983:19). Fergusson firmly believed that ancient Indian architecture 'permits us to know exactly the religion, the art and civilization of the people who built it' (as quoted by Chandra 1983:17). He first of all familiarized himself with the principles of ancient Indian architecture to such an extent that he 'could read in the chisel marks on the stone the ideas that governed the artist in his design' (as quoted in Chandra 1983:17).

Between 1829 and 1847 Fergusson undertook extensive architectural surveys in the country. These prolonged studies involving preparation of careful notes, drawing of sketches, making of plans and, above all, hard thinking culminated in two major publications (1845; 1876). He achieved such a high degree of mastery in the subject that he could claim that 'if anyone would produce me a set of photographs of any ancient building in India, I would tell him within fifty miles of where it was situated, and within fifty years of when it was built' (Fergusson 1974:2).

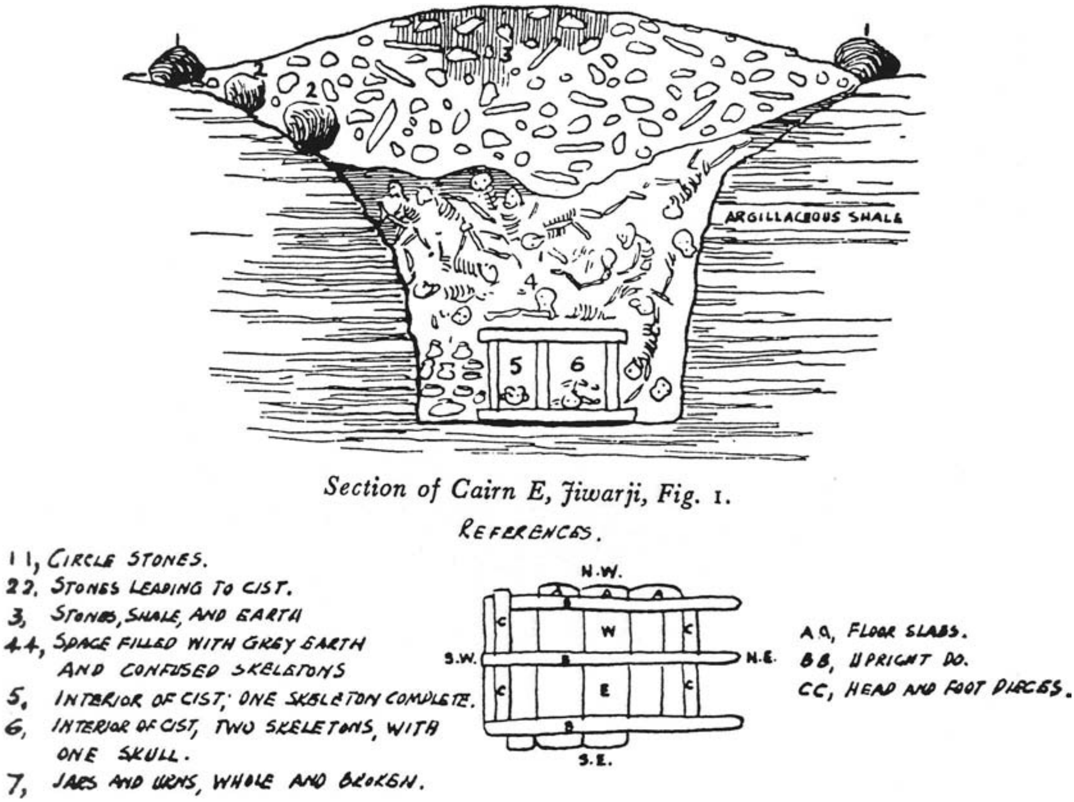


Figure 6.7 Meadows Taylor's stratigraphical record of Jewargi in the Deccan (After Meadows Taylor, 1941, *Megalithic Tombs and Other Ancient Remains in the Deccan,* Hyderabad: Hyderabad Archaeological Department)

With the help of knowledge thus acquired Fergusson classified the monuments as belonging to the Buddhist, Hindu, Jain and Muslim periods. He also recognized the existence of distinctive styles (Dravidian, Indo-Aryan, Himalayan, Chalukyan, etc.), each characterizing well-defined distinctive groups. In his own words:

> Nowhere are the styles of architecture so various as in India, and nowhere are the changes so rapid, or follow laws of so fixed a nature. It is consequently easy to separate the various styles into well-defined groups, with easily recognized peculiarities, and to trace sequences of development in themselves quite certain, which, when a date can be affixed to one of the series, render the entire chronology certain and legible.
>
> (Fergusson 1974:2).

Fergusson's lectures in England in the 1860s, espousing the sequence of styles represented by the cave temples of India, made scientists like T.H.Huxley (as quoted in Allchin 1961:242) wonder whether 'Darwinismus' had not percolated fields, where 'you least expected it'.

Alexander Cunningham (Fig. 6.8) was a military engineer by profession and served in eastern India. He developed a deep interest in Indian archaeological remains even while in service. On his retirement he submitted a memorandum to the government in 1861, in which he made the following indictment:

> During the one hundred years of British dominion in India, the Government has done little or nothing towards the preservation of its monuments, which, in the almost total absence of any written history, form the only available sources of information as to the early condition of the country. Some of these monuments have already endured for ages and are likely to last for ages still to come; but there are many others which are daily suffering from the effects of time, and which must soon disappear altogether, unless preserved by the accurate drawings and faithful descriptions of the archaeologist.
>
> (Cunningham 1871:iii)

Cunningham's plea had a compelling effect on Lord Canning, the Governor-General, who accepted the need for the creation of a separate department for this purpose, and thus there came into existence what is now known as the Archaeological Survey of India. It was entrusted with the task of

> an accurate description—illustrated by plans, measurements, drawings or photographs, and by copies of inscriptions—of such remains as most deserve notice, with the history of them so far as it may be traceable, and a record of the traditions that are retained regarding them.

Cunningham himself was appointed Director of this newly created department (see Imam 1966).

Using the travelogue of the Chinese pilgrim Hiuen-Tsang (seventh century AD) as his guide (Fig. 6.9) and also drawing upon his previous knowledge of some of the ancient sites in northern India, Cunningham carried out annual surveys (with one gap of four or five years) until his retirement in 1885. He adopted what may be called a topographical approach and criss-crossed practically the whole of northern India from Kashmir in the north to the Narmada in the south and from Gujarat in the west to Bengal in the east. Preparation of detailed site maps as well as plans and elevations of buildings, recording of local traditions about ancient sites, use of evidence provided by coins, images and inscriptions, sinking a trial trench into the top of a mound whenever he felt necessary, and photographic documentation were the main field strategies adopted by Cunningham. The annual reports published by him, in the words of



Figure 6.8 Sir Alexander Cunningham (1814–1893), first Director of the Archaeological Survey of India (After S.Roy, 1961, *The Story of Indian Archaeology 1784–1947,* New Delhi: Archaeological Survey of India)

the later Viceroy Lord Curzon, 'constitute…a noble mine of information in which the student has but to delve in order to discover an abundant spoil' (as quoted by Roy 1961:60). Regardless of whether there is justification in the criticism that Cunningham's work promoted missionary interests in India and furthered the divide-and-rule policy for governing the country, there is no doubt that he laid firm foundations for early historical archaeology in India. It was a fair assessment of Cunningham's achievement when Wheeler (1955:180) wrote:

> his personal survey work…was of an outstanding range and quality. Today a surveyor in Cunningham's boots would be expected to operate primarily by aeroplane, train and car. Save for a rare train, Cunningham had none of these advantages. He used his boots for the purpose for which they were made, with interludes of saddle and bullock-cart. In consequence he saw the countryside, not through gaps in clouds or engine-smoke, but at close and familiar range, stopping a while to commune and converse when the spirit moved him as it often did…. His name ranks high in the select sodality of discoverers; there was genius in his composition.

Robert Bruce Foote (Fig. 6.10) of the Geological Survey of India accomplished an equally outstanding feat in respect of prehistoric archaeology. Foote landed on Indian soil in 1858. Influenced as he was by the discovery and dating of the stone tools in the Somme valley of France, he started 'looking out for possible similar traces of human art in South India' (Foote 1916:v). Beginning with his pioneering discovery of stone implements near Madras in 1863, Foote found over 450 prehistoric sites in southern India and Gujarat in the course of his geological surveys spread over three decades. He brought the richness of Indian prehistoric material to the notice of the scholarly world by writing articles both in and outside India and displaying actual specimens in European exhibitions. His collections of stone tools, pottery and other objects were donated to the Madras Government Museum. He recognized the existence of three distinct phases in the prehistoric past of South India—the Palaeolithic, the Neolithic and the Iron Age.

The catalogue which Foote (1916) prepared on his collections is full of insights into the settings of archaeological sites in relation to topographical features, raw material sources, vegetation types and other geographical features, thereby betraying Foote's eagerness to understand past cultures against the background of their environmental settings. Considering these multi-faceted achievements of Foote, Chakrabarti's (1979) criticism that he failed to recognize the existence of a mesolithic phase in India does not have much importance.

After Cunningham's retirement in 1885 archaeology in India fell into such bad shape that field research had practically come to an end and the post of director itself was eventually abolished. It is Lord Curzon, Viceroy from 1889 to 1905, who must be given the credit for injecting new life into this field of study. After personally examining the whole situation, he concluded in 1900 that 'it was

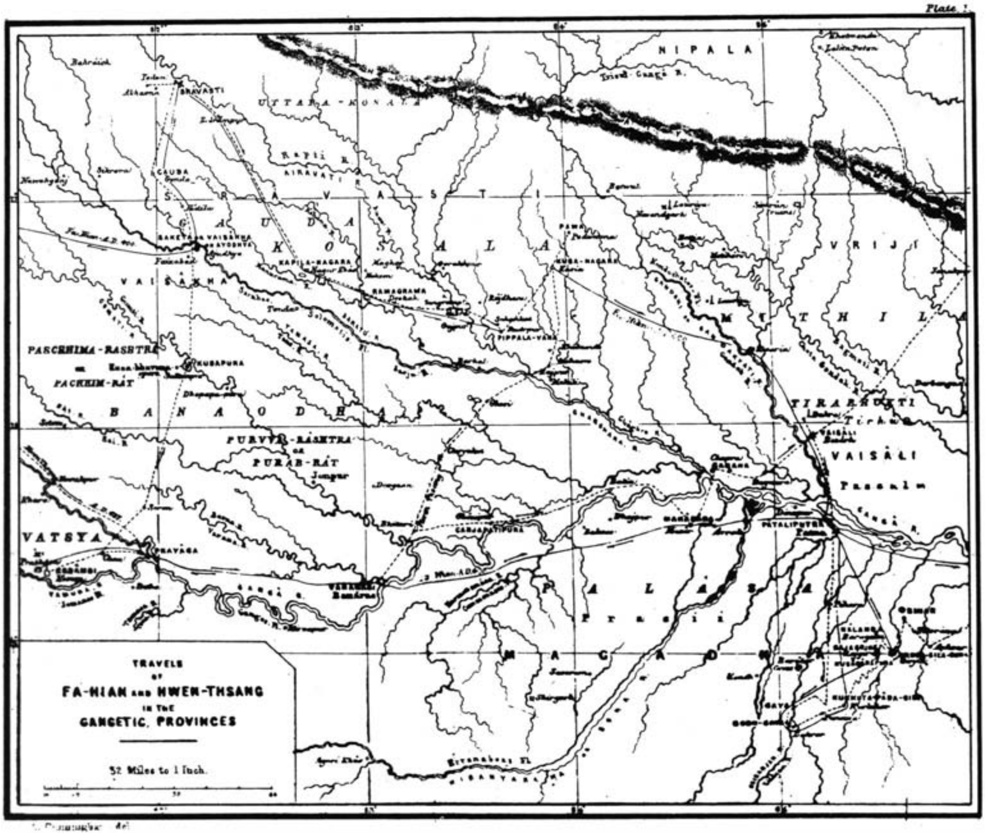


Figure 6.9 Alexander Cunningham's 1871 map of the middle Gangetic basin, showing the places visited by the Chinese pilgrims Fa-Hien and Hiuen-Tsang (After Alexander Cunningham, 1871, *Four Reports Made During the Years 1862, 1863, 1864, 1865,* vol. 1, Calcutta: Archaeological Survey of India)

impossible to conceive a system more chaotic or futile in practice'. He found fault with the delegation of the work to provincial administrations, illogical division of the country into circles and neglect of conservation of monuments, and also highlighted the lack of centralized leadership. Like Lord Canning before him, Curzon also blamed the imperial government for its inadequate provision of funds and attention given to antiquarian work. Adopting a holistic policy towards India's cultural heritage, he emphasized that:

> It is in my judgement equally our duty to dig and discover, to copy and decipher and to cherish and conserve.
>
> (Roy 1961:83)



Figure 6.10 Robert Bruce Foote (1834–1912) (After S.Roy, 1961, *The Story of Indian Archaeology 1784–1947,* New Delhi: Archaeological Survey of India)

As part of a plan to reinvigorate the Archaeological Survey, Lord Curzon revived the post of director and John Marshall was brought out from England to fill it in 1902. Marshall dominated Indian archaeology for the next three decades to such an extent that this period may safely be called the Marshall epoch. Befitting his previous experience of working on largescale excavations in Greece, southern Turkey and Crete, Marshall set himself the task of:

> recapturing the total culture of India in past ages with their cities and streets, their furniture and tools, their arms and weapons, their ornaments and jewels, their seals and coins, and their laws and customs.
>
> (Roy 1961:90)

In addition to creditable achievements in various branches such as conservation, organization of museums, publications, epigraphy, numismatics and even the stimulation of interest in archaeology among the princely states (for details, see

Cumming 1939), Marshall initiated an extensive programme of excavations. He and his colleagues not only discovered the sites of Mohenjo-daro and Harappa, but conducted large-scale excavations there, and the Indus civilization was born in archaeology. Excavations were also conducted at early historical sites such as Taxila, Vaisali, Nalanda, Rajagriha and Sarnath, and these provided the much needed archaeological evidence for historical reconstructions hitherto based on written texts. Notwithstanding certain criticisms levelled against his excavation techniques by later researchers, Marshall thus translated into reality the paradigm of the reconstruction of past ways of life. The enactment of the Ancient Monuments Act of 1904 and the recruitment of several Indians to high positions in the Survey are the other major achievements of the Marshall era (Marshall 1939). Viewing these various achievements of the Survey in studying the past of India, it is not surprising that nationalist leaders like Pandit Motilal Nehru were impressed by its efforts and even volunteered support for its activities (Marshall 1939:33).

During the 1930s archaeology once again sank into a disorderly state. As a result, the Secretary of State finally appointed a one-man committee of Sir Leonard Woolley to report on the work of the Survey. His report, submitted in 1939, was critical of many aspects of the work being carried out by the Survey, particularly the policy of taking on a series of small excavations without any element of planning (Woolley 1993; see also Possehl 1993). It was against this background that Brigadier (later Sir) Mortimer Wheeler was brought on to the scene. Wheeler's archaeological thought was as much influenced by the humanist-orientated views of history propounded by Collingwood and Haverfield, as by the improvements made in field-techniques by Pitt-Rivers in Britain (see Hawkes 1982).

Wheeler's short term of office lasting four years (1944–48) was marked by a series of developments which would normally take forty years—the organization of a training school in field archaeology at Taxila, which was probably the first of its kind in world archaeology, the stimulation of interest in archaeology in the Indian universities, changes in conservation policy, the establishment of a national museum, the publication of the highly regarded, but now defunct, journal *Ancient India* and, above all, the introduction of strategic or problem-oriented fieldwork (for details see Wheeler 1955; 1976; Clark 1979). Strategic planning introduced by Wheeler (1946; 1949) produced remarkable results in Indian archaeology for almost a decade. To start with, it led to Wheeler's own excavations at Arikamedu and Brahmagiri aimed at deciphering the proto-historic and early historic culture-sequence in southern India, and at Harappa, to ascertain the existence of fortifications.

Thanks largely to the momentum provided by Wheeler, the 1950s and 1960s witnessed a rapid expansion of archaeological studies in India, pursued by both government (central and provincial) and university departments. Researchers basically continued to operate within the traditional culture-historical framework inherited from the colonial period and, to a large extent, they still continue to do

so. The principal components of this framework included the building up of local or regional culture-sequences, use of the 'Childean' notion of cultures as recurring assemblages of distinctive artefact types, the tendency to equate cultures so recognized with ethnic groups, imprecise use of core concepts such as culture type, site and region, a lack of awareness of the importance of higher-order concepts such as time, causality, probability and explanation, the use of present-day administrative/political divisions as regional units for investigating the archaeological record, unrestrained coining of new culture-complexes on the basis of pottery, fabrics or stone-tool types, and simplistic use of diffusion/migration for explaining culture change (and see Evans 1995). Both Indian and foreign archaeologists often invoked invasion/diffusion as tools for explaining away the origins of fully-fledged archaeological cultures ranging in age from the Lower Palaeolithic to the early historic period as well as individual traits concerning pottery, technology and other aspects. Africa, West and Central Asia and Europe were the favourite source areas (Chakrabarti 1988:18–34).

The culture-historical approach probably found its best expression in prehistoric research. Using as models research undertaken between the 1930s and 1950s, on the Soan culture-sequence, the cultural-climatic sequence of the south-eastern coast of India and palaeo-environmental studies on the alluvial deposits of western India, Sankalia and his colleagues, as well as researchers from other institutions, carried out a number of regional studies in various parts of the country (Sankalia 1974). In these studies the emphasis was on the formulation of regional culture-sequences based on the finding of stone artefacts in successive gravel and silt layers making up river-sections, and the correlation of cultural phases with wet or dry climatic episodes inferred in a simplistic manner from the occurrence of gravel/silt deposits respectively. This geological approach to the Stone Age past, oriented in terms of river sections and secondary sites, was obviously modelled on work undertaken in western Europe and became the dominant paradigm in Indian prehistoric research. It is not altogether dead even now.

Some archaeologists, such as Subbarao (1958), sought to elevate the study of the role of geographical factors in Indian archaeology by drawing attention to the immense geographical and cultural diversity existing in the subcontinent (see Chakrabarti 1988:50–64). He introduced the notion of nuclear areas, areas of relative isolation and areas of isolation. However, no real progress could be made in the understanding of past human adaptations on account of major theoretical and methodological flaws in the culture-history paradigm itself.

THE 'NEW ARCHAEOLOGY', AND BEYOND

Malik (1968; 1971; 1975) was the first to call for changes in the classificatory descriptive approach outlined above. His plea to archaeologists to appreciate the importance of conceptual analysis and to adopt a holistic, anthropological approach to the archaeological record fell on deaf ears for some inexplicable

reason. It was not until the mid–1970s that Indian archaeology began to experience the impact of stirrings created on both sides of the Atlantic by the emergence of the New Archaeology. Sankalia examined in detail the relevance of the writings of Binford and Clarke in the Indian context, culminating in the publication of *The New Archaeology: its scope and application to India* (1977). In 1986 Deccan College, Poona, conducted a month-long course of lectures (with Binford as one of the resource persons) on the New Archaeology for college and university teachers. In 1988 the Indian Council of Historical Research, New Delhi, organized a seminar on the 'New Archaeology and India', in which over a dozen papers were presented, seeking to examine the application of the New Archaeology to various branches of Indian archaeology. These publications and gatherings created a degree of awareness of global developments among Indian archaeologists. Chakrabarti (1989) bypasses epistemological issues and instead concentrates on topics such as the role of geographical factors, use of diffusion/ migration as a heuristic device for explaining culture change and the emergence of agriculture, while Paddayya (1990) discusses the developments of the last three decades in Anglo-American archaeology and situates them within an Indian context. The 1970s and 1980s saw the emergence of a number of studies by Indian scholars in prehistory and proto-history who adopted, with varying degrees of success, some of the major insights provided by the New Archaeology.[3] These included the conception of cultures as adaptive systems rather than trait-lists of items, the selection of regional units in terms of natural features, the need for intensive field surveys instead of the previous hit-and-run type of exploration, the need to locate primary sites, the adoption of a settlement-system approach to the archaeological record, and the use of ethno-archaeological models for attempting such settlement-system reconstructions.

At the same time as these developments in archaeology, certain major changes took place in the approaches to ancient Indian history. Some researchers endeavoured to move away from the long-standing preoccupation with political history to a holistic approach, emphasizing the importance of economic and socio-cultural factors. This trend was influenced by functionalist, Annales and Marxist approaches (e.g., Kosambi 1956; Sharma 1966; Thapar 1984a; Thapar 1984b; Sharma 1985; Thapar 1992). Some of these even envisaged a greater role for archaeological evidence in historical reconstructions (e.g., Thapar 1984a:193–210; Sharma 1987).

Some researchers have begun using certain aspects of post-processual archaeology for studying the Indian archaeological record. Adopting the cognitive version of post-processual archaeology, Miller (1985) has studied the pottery being manufactured and used today in the Dangwara village of central India in order to detect distinctions of caste, family size and wealth. Fritz (1986: 1) has used the symbolic component of post-processualism for interpreting the layout of the medieval town of Vijayanagara. According to him, the layout of the royal residence, the spatial positions of temples and other structural features, the circum-ambulatory depiction of movement around the royal residence,



Figure 6.11 Ananda K.Coomaraswamy (1877–1947) (After Pramod Chandra, 1983, *On the Study of Indian Art,* Cambridge, Mass.: Harvard University Press)

symbolizing *pradakshina* in a temple, and the mythological connections were all designed to evoke associations with the legendary capital of Ayodhya and the epic hero Rama. Indeed, the Vijayanagar capital served as the meeting-ground for god and king. 'King and god were the focus of the city; they paid homage to each other, and by radiating their energies outwards, they gave form, harmony and plenty to the empire' (Fritz 1986:53).

In this connection it is important to remember that awareness of the symbolic dimension of the archaeological record was already foreshadowed in the early part of this century in research on Indian art and architecture (e.g., Coomaraswamy from the 1920s to the end of the 1940s). For example, Coomaraswamy (Fig. 6.11) viewed the Hindu temple not only as a building giving shelter to image and worshipper, but also as the image of the cosmos, the house of God and His body, representing in its parts the drama of disintegration and reintegration, which is the essential theme of Indian myth and its ritual enactment in sacrifice. He thus elevated the study of the temple beyond the spatio-temporal framework and brought out its inner meaning and the very reason for its existence (Coomaraswamy 1938). This theme was subsequently developed further by Kramrisch (1946; 1975), who treats the temple as the *Purusha* (a metaphorical representation of the creative force in the cosmos). Similarly, Coomaraswamy (1927) refuted the Greek origin postulated by earlier researchers for the Buddha

image of the Gandhara school and instead stressed that the *nirvana* posture is rooted in Indian tradition. He emphasized that the question was not one of the Gandhara sculptor making 'an Apollo into Buddha but of making a Buddha into Apollo'.

Empathy and contextual analysis have emerged as the major aspects of post-processual archaeology's approach to the past. These mental operations are also foreshadowed in the writings of Coomaraswamy. While underscoring their relevance to the study of Buddhist iconography, he writes:

> In order to understand the nature of the Buddha image and its meaning for a Buddhist, we must, to begin with, reconstruct its environment, trace its ancestry and remodel our personality. We must forget we are looking at 'art' in a museum and see the image in its place in a Buddhist church or as part of a sculptured rock wall and, having seen it, receive it as an image of what we are ourselves potentially. Remember that we are pilgrims come from some great distance to see God, that what we see will depend upon ourselves. We are to see, not the likeness made by hands, but its transcendental archetype, we are to take part in a communion…The image is one of Awakened: and for our understanding, who are still asleep. The objective methods of 'science' will not suffice, there can be no understanding without assimilation, to understand is to have been born again.
>
> (Coomaraswamy 1986:147–8)

ARCHAEOLOGY AND THEORY IN INDIA TODAY

Despite the introduction of the changes noted above, the general attitude of Indian archaeologists to theoretical debates, as understood in their broadest sense, continues to be one of indifference and sometimes outright cynicism. It is not uncommon to come across the remark from Indian archaeologists that theoretical debates have 'gone over their heads'. This is an unfortunate situation and all-out efforts must be made to change it.

What are the factors that lead to this indifference towards theoretical debate? The principal reason is the notion prevalent among most researchers that the discipline is a fact-gathering enterprise and hence there is no need for epistemological discussion (Paddayya 1985:14). A false understanding of the very definition of science, the notion that archaeology is an inferior kind of science, and ignorance of developments in other scientific disciplines are the chief factors promoting this 'frog-in-the-well' attitude. One may add to these factors certain other considerations pointed out by Wheeler (1955:124–7), such as poor library facilities, lack of scope to learn other European languages such as French and German, a disproportionate number of holidays in a given year, lack of incentive, the enervating climate and the peculiar conditions of family life in India leaving no time for research, which all make the young Indian scholar 'lose

the ability and urge to pursue his chosen subject save as a business routine. He loses, or fails to acquire, the habit of research' (Wheeler 1955:214–15).

There is however a silver lining. At places like Poona the response from students to the inclusion of general theoretical topics for teaching purposes has been positive. This in turn has led to some qualitative changes in the selection of topics for doctoral research. In lieu of earlier all-embracing topics such as the archaeology of Stone Age cultures of a particular district or province, topics covering themes such as human adaptation, calling for a more dynamic approach to the use of geo—and biological sciences, settlement-system studies, middle-range research or investigation of site-formation processes and the critical study of the history of archaeology are now viewed as meaningful topics, and are taken up for investigation. It is to be hoped that this trend will accelerate.

Nevertheless, in a country such as India, it is particularly necessary to keep the complementary nature of differing theoretical approaches in mind. Here the archaeological record not only possesses great time-depth, but exhibits tremendous diversity in its make-up (Paddayya 1990:Ch. 3). The level of prior knowledge of the archaeological material of the area and period concerned, as well as the intellectual make-up of the archaeologist himself or herself, rather than blind allegiance to a particular theoretical orientation, should be seen as the guiding factors in the choice of options for a particular research orientation. In the case of regions which are still archaeologically *terra incognita* the application of the culture-historical approach has enormous significance. In those areas where a skeletal framework is already available, perspectives developed by processual and post-processual archaeologies are particularly useful.

Use of indigenous epistemological traditions for studying the past is an area of great potential and it is unfortunate that as yet not even an exploratory attempt has been made by historians and archaeologists to investigate it. Even superficial analysis shows that the Indian conception of knowledge is a very elaborate one (e.g., Datta 1967). The Sanskrit word for cognition in general is *jnana* and valid cognition is called *prama*. The Hindu tradition recognizes a number of sources *(pramana)* of knowledge: perception, inference, authority, knowledge based on similarity *(upamana),* postulation *(arthapatti),* non-cognition, intuitive knowledge *(pratibha)* and unbroken tradition *(itiha)*. Valid cognition is considered to be free from doubt *(samasya),* indefiniteness *(anadhyavasaya)* and error *(bhrama)*: it therefore reveals things as they are and furnishes the basis for successful activity.

In Hindu epistemology the art of reasoning goes back to the *Upanishads*. The *Chandogya Upanishad* holds that reasoning with words *(vakovakya)* must be supported by experience and spiritual insights. In later texts like the *Manusmriti* greater appreciation is expressed for this faculty of the human mind. It is however in the *Nyayasutra* of Gautama (suggested age ranging from fourth century BC to fourth century AD) that we find its most elaborate expression. According to Gautama, doubt *(samasya)* is the chief incentive to philosophical inquiry. He

then goes on to prescribe an elaborate process for removing doubt. To put it in the words of Datta (1967:132):

> One must consider carefully the pros and cons *(paksha-pratipaksha)* and ascertain the true nature of things. For this purpose one is advised to take the help of all valid sources of knowledge, use (and avoid conflict with) previously estabished theories *(siddhanta),* use examples *(drishtanta)* which are acceptable to all, employ the five-step method of discovery and proof *(pancavayavanyaya),* use the indirect hypothetical or postulational methods of strengthening the conclusion *(tarka),* and also take care to avoid five kinds of material fallacies *(hetvabhasa),* three kinds of quibbles *(chala),* twenty-four kinds of false analogies *(jati),* and twenty-two kinds of self-stultifying steps, which could cause defeat in debates.

This elaborate method was not merely a matter of philosophical discourse, but is meant to cover all domains of knowledge including the empirical sciences (see Seal 1915:244–95). In fact, on reflection, one realizes that it is for all practical purposes identical with the scientific method as conceived in western philosophy.

Another area of great potential is that of indigenous notions of concepts such as space, causality and explanation. Of special interest is the Buddhist theory of causation known as *paticca samuppada,* or dependent co-arising, which resembles general systems theory (see Macy 1992; Jayatileke 1980). In contrast to the linear notion of causation, this doctrine of *paticca samuppada* enunciated by the Buddha treats an event not as the result of one cause but as the concatenation of diverse, potentially causative factors into a unique relationship with one another. Thus causality is seen as reciprocal and encompassing all phenomena, human and natural. The Buddha equated the doctrine with the *Dharma* itself, i.e., the orderly process involved in the working of things. Causation thus refers not merely to things and persons but to the processes connecting them together as such.

Post-processual archaeology treats the archaeological record as a text. We are further told that the meanings embedded in it are not to be deciphered in a matter-of-fact way but read in a continuous fashion. This has led to the growth of interpretative trends in contemporary archaeology. In this connection it is important to realize that the tradition of interpretation also goes back to an early period in India. This was applied to the study of ancient texts and is known as *tika* (elucidation) and *bhasya* (commentary) (Kapoor 1991). Also noteworthy is the use of hermeneutics for understanding the meanings of ancient texts ranging from Vedic literature to the writings of medieval saints (Arapura 1986; Sundara Rajan 1991). Employing this perspective in his study of the writings of the saints of Maharashtra, Lele (1987) came to the conclusion that the writings of Jnaneshwar and Tukaram not only present a critique of the contemporary social

order but at the same time offer an all-encompassing blueprint for changing it (see also Sardar 1969).

It is imperative to explore the usefulness of such indigenous interpretative paradigms as a methodological strategy for understanding the country's archaeological record.

While historians and archaeologists will continue to use deconstruction and colonial discourse analysis for endlessly debating the motives of European orientalists, one thing about the outcome of their long and laborious efforts is certain: Europe's rediscovery of India's past contributed significantly to the growth of both the Renaissance in Bengal and other areas in the nineteenth century and the nationalist movement (see Kopf 1969). The nationalist leaders, while they complimented the British government on its efforts to resuscitate the country's past, ironically enough used the results to incite the people against the colonial masters. India's past as revealed by European efforts promoted the growth of feelings of national pride.

THE PRESENT AND THE FUTURE

In the contemporary context, the scene is one of total chaos. The December 1992 incidents at Ayodhya in northern India are yet another instance of the distorted use of the past. One could easily, and rightly to some extent, blame in some regard the British for their policy of 'divide and rule' and British scholars for dividing up the history of India on a religious basis—a Hindu period, a Buddhist period, a Muslim period and a Modern (British) period. Yet a more mature approach requires that, instead of bewailing this legacy of British scholarship, Indians take concrete steps to educate society about the past. Precious little has been done over the last forty-five years. The result is the indiscriminate use of the past by interested groups for their own ends (and see Rao 1994). It is one thing to name trains, dams and aircraft after ancient rulers, dynasties and monuments—this has surely allowed the people to identify themselves with the past and develop some pride in it. It is an altogether different matter, however, when some of the politico-religious groups have been allowed to use some aspects of the past to promote their narrow interests. The Ayodhya incident is an example of this kind, and there are many other instances threatening to disrupt social order.

In the Ayodhya affair historians and archaeologists have played a role which is not above suspicion. Instead of avoiding prejudice and bias in their outlook, as encouraged by Kalhana centuries ago, and accordingly educating society to appreciate that a multi-culture site such as Ayodhya belongs to the whole nation and not to any particular religious community, they have grouped themselves into two camps and sided with one or the other politico-religious group. To an extent the whole build-up of the Ayodhya situation owes its origin to the differing versions of 'historical facts' proffered by the two camps of historians and archaeologists. A third-party approach, combining pragmatism with wisdom

of the Buddha, would have been helpful in preventing northern India from turning into a cauldron. India's past and its students, instead of serving as a source of enlightenment for society, have become a burden on it. It is not unreasonable that the ordinary citizen has now started expressing doubts about the relevance of both to society.

My own belief is that the situation can be changed by making the study of the past an important aspect of the education of the public. A non-partisan understanding of the past on the part of the ordinary citizen, and his/her ability to appreciate the universality of human culture behind the facade of its spatio-temporal diversity, are the best insurance against any abuse of the past. It is in this direction that co-ordinated attempts will have to be made. In this regard, I believe that the ideas developed by Nehru (1960) are very helpful as a starting-point. Nehru did not study India's past from an academic point of view (for a misconceived analysis of Nehru's notion of history, see Gokhale 1978). He viewed it instead as a vantage-point for looking at, and understanding, the present:

> if it [the past] does not [touch on the present], then it is cold, barren, lifeless.
>
> (Nehru 1960:22)

An objective understanding of the past should be treated as an important element of the promotion of what Nehru called the scientific temper of mind. He defined it as 'that critical faculty in considering problems, that evenness of temper, that objective way of looking at things' (see Singh 1986:38). Considering the fact that the Indian people had been steeped in superstitions, prejudices and perversions of various kinds for centuries, Nehru envisaged for the scientific temper a role in national regeneration as large and crucial as the one he carved out for science and technology.

The question of the social relevance of the study of the past is one which historians and archaeologists in India can no longer overlook. Public attitudes towards the study of the past, the role of the mass media and museums in bringing knowledge of the past to society at large, and the use of the study of the past for discussion and the display of power in professional institutions are other questions which, sooner or later, Indian archaeology will have to confront.

Finally, to return to Europe, one may ask what understanding has been gained in that continent by European studies of India's past? In 1841 Edgar Quinet coined the highly evocative phrase 'The Oriental Renaissance' to refer to the 'revival of an atmosphere in the nineteenth century brought about by the arrival of Sanskrit texts in Europe, which produced an effect equal to that produced in the fifteenth century by the arrival of Greek manuscripts and Byzantine commentators after the fall of Constantinople' (Schwab 1984:11). In 1882 the Sanskritist, Max Mueller, took stock of the knowledge that had accrued from the study of Sanskrit and other ancient Indian languages, and ungrudgingly gave to India the esteemed status of being the land where

> the human mind has most fully developed some of its choicest gifts, has most deeply pondered on the greatest problems of life, has found solutions to some of them…
>
> (Mueller 1919:6)

He went on to say that Indological studies offer a corrective to Europe

> in order to make our inner life more perfect, more comprehensive, more universal, in fact more truly human…
>
> (Mueller 1919:6)

and concluded that the study of Sanskrit and related languages of India had

> widened our [European] views of man, and taught us to embrace millions of strangers and barbarians as members of one family…
>
> (Mueller 1919:30)

Did the study of India's archaeological heritage exert any influence of its own? The answer is once again positive. The magnificent discoveries of the last century, involving scores of prehistoric and historic sites, won for the country a high degree of esteem and respect among European archaeologists—an attitude of mind which many parts of Asia and Africa clearly failed to evoke. Reminiscing about his experience of Indian archaeology, Wheeler (1976:66) wrote:

> In India it is possible to dig almost anywhere below a living level and to discover the vestiges of civilization layer by layer. That is not of course true of a great many regions of the world. Large expanses of Africa, for example, would be singularly unresponsive to a crude test of this kind.

Indeed, the discovery of the Indus civilization made India a respected member of the small number of lands that gave birth to true civilized life. In India, at least, the discipline of archaeology has served the country well, allowing it to take its rightful place as one of the oldest and most interesting regions of human endeavour.

NOTES

1 Philips (1961) was the first publication to examine the historiographical tradition in India; Pathak (1966) was the first book devoted exclusively to this topic. Also of interest are Warder (1959; 1970) and several research papers published in Indian and foreign sources by Romila Thapar (1968, 1972, 1976, 1978—all reprinted in Thapar 1984a; and 1977, 1986—both reprinted in Thapar 1991; 1992). The summary given in this chapter is to a great extent based on her writings.

2 There are several publications devoted to the history of archaeology in India. Cumming (ed.) (1939) was the first book of its kind. In 1953 *Ancient India* included six important papers dealing with the history of investigations in various branches of archaeology. More recent books include Roy (1961) and Chakrabarti (1988; 1989).

Particularly insightful analyses are those by Cunningham (1871), Marshall (1939) and F.R.Allchin (1961).

3 Examples of such studies in prehistory include Misra (1973; 1976; 1978; 1987) in central India and Rajasthan; Murty (1974; 1981; 1985) and Raju (1988) in Andhra Pradesh; Paddayya (1982; 1991) in Karnataka; Mohanty (1989) in Orissa; the University of Allahabad in the Ganga Valley (Sharma 1973; Sharma, Misra, Mandal, Misra & Pal 1980); and Pant & Jayaswal (1991) in Bihar.

Examples of such studies in proto-history include the extensive work of Sankalia and his colleagues on the chalcolithic cultures of Maharashtra (Dhavalikar, Sankalia & Ansari 1988; Dhavalikar 1988); Makkhan Lal (1984) in the Gangetic Doab; Venkatasubbaiah (1992) on the neolithic culture of South India; and Deo (1985) and Murti (1989) on the megalithic culture of the Iron Age.

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I dedicate this paper to the memory of Sir William Jones on the occasion of the bicentenary of his death.

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CHAPTER SEVEN
THE 'ABORIGINALIZATION' OF AUSTRALIAN ARCHAEOLOGY
The contribution of the Australian Institute of Aboriginal Studies to the indigenous transformation of the discipline

STEPHANIE MOSER

INTRODUCTION

In seeking to document the development of world archaeological traditions it is fundamental to examine the contribution of indigenous minorities. While the theme of 'domination and resistance' has been given some attention in the examination of how regional archaeologies have developed (Miller, Rowlands & Tilley 1989), the effect that indigenous involvement has had in the construction of disciplines remains poorly documented. Until recently relatively little has been written about the contribution of Aboriginal people to the development of archaeology as a discipline in Australia (see now Bowdler 1988; Flood 1989; Creamer 1990; Pardoe 1990; Pardoe 1991a; Pardoe 1991b; Pardoe 1992; Clarke 1993; Tacon n.d.; Williams n.d.). While there is little doubt that the involvement of Aboriginal people in Australian prehistoric archaeology has had an impact on the way in which archaeologists go about their work, the extent to which this involvement has affected the nature of the discipline is less clear.

An early discussion on how Aboriginal involvement transformed the aims of academic archaeology was presented by the former principal of the Australian Institute of Aboriginal Studies (AIAS), Peter Ucko (1983). Ucko examined how the research objectives of the Prehistory Advisory Committee at the Institute changed as a result of Aboriginal participation. He claimed that 'Aborigines have in the past few years transformed the academic discipline of archaeology from an aseptic one of purely scientific enquiry into a humane investigation of the past development of cultures whose practitioners still live in the country of their origin' (Ucko 1983:22). The aim of this chapter is to document how Aboriginal demands to be involved in the study and management of their heritage have affected the nature of the discipline. While state bodies, federal agencies, university departments and numerous individuals have been involved in implementing Aboriginal perspectives in Australian archaeology (see McBryde 1989), I have chosen to focus on how one particular organization facilitated the involvement of Aboriginal people in Aboriginal studies, and how this affected the practice of archaeology in the country. This chapter investigates how the federal body, the AIAS (set up in 1964), fostered the recognition and

incorporation of Aboriginal interests into the very young and under-developed discipline of Australian archaeology.

THE DEVELOPMENT OF THE AIAS

The seeds of the Institute were first sown in 1959 when a proposal for a national research effort on the Aborigines was sent to the vice-chancellor of the Australian National University by a minister of parliament (Wentworth 1959). In response to this proposal, a national conference on Aboriginal studies was held in Canberra and scholars working in anthropology, prehistory, human biology, material culture and linguistics came to present papers on their fields of study (Horton 1986). The '1961 Conference on Aboriginal Studies' held at the Australian National University was a landmark in Aboriginal studies because it was the first time that people working in the field had all gathered together to discuss issues of common interest. The idea that Aboriginal people were dying out and that what remained of their cultural life needed to be recorded was the central underlying theme. As a result a sense of urgency in studying Aboriginal cultural life was communicated in the papers delivered at the conference. The main organizer of the conference, William Stanner, claimed that, 'I felt that all who took part had a sense of making history' (Sheils 1963:xiv). McBryde (1986:19), who attended the conference, said that it created 'winds of change' through the universities and museums. Following the conference, in December 1961, the Prime Minister appointed an Interim Council to make recommendations towards the establishment of a national research organization with the title of the 'Australian Institute of Aboriginal Studies'. This Council expressed the 'intention of affiliating the Institute with international bodies, and of promoting harmonious relations with State universities, museums and other institutions working in the field of Aboriginal Studies' (*AIAS Newsletter* 1963, 1:5). With the setting up of this institution came a critical turning-point in the history of Australian archaeology. For this discipline in particular, the Institute became fundamental to the construction of a self-sufficient profession.

While the Interim Council worked towards establishing the new Institute, in 1964 the 'Australian Institute of Aboriginal Studies Act' was created, which established the Institute as a body corporate. The role and function of the Institute was outlined—it was to promote Aboriginal studies, to publish or assist in the publication of such studies, to encourage and assist cooperation amongst universities, museums and other institutions concerned with Aboriginal studies, and to assist the above bodies in training research workers or directly assisting persons engaged in Aboriginal studies (*AIAS Newsletter* 1964, 1:15). The staff of the newly created AIAS consisted of a principal with secretarial assistance, documentation resources, field equipment, sound recording officers and a librarian. A newsletter was to be issued every six months. The first principal to be appointed was Fred McCarthy. On taking up his position McCarthy (1965: 307) emphasized that the Institute was 'not only a source of funds for research,

but a co-ordinating and advisory body building up slowly a corpus of information available nowhere else about our Aborigines'. Decisions at the Institute were made through a council which included leading figures in the field of Aboriginal studies from all subject areas and who were working in the universities and other state and federal organisations. Of crucial importance to the work of the Institute were the specialist advisory panels which were made up of around ten members from the respective subject fields.

With its brief to assist and support the disciplines working in Aboriginal studies, the Institute offered much in the way of assisting undeveloped disciplines, such as prehistory, to set up their own institutional infrastructure. For the more established disciplines, such as social anthropology, the Institute would offer a more supportive role and provide much-needed funds for research. When the Institute was set up, it forged a close relationship with Australian archaeology. While part of the explanation lies in the fact that the principal, McCarthy, was himself an archaeologist who was very aware of the needs of the discipline (see Moser 1994), it also lies in the fact that prehistory did not really exist as an independent field. For instance, there were no official posts for Australian prehistorians in universities, there was no specialist journal, no national or professional associations, and extremely limited resources for carrying out work. In the early 1960s, the small group of newly appointed professional archaeologists working in the universities and museums drew heavily on Institute resources to help build the foundations of their discipline. The community of Australian archaeologists benefited from the Institute's work in funding research; organizing conferences and meetings; creating a newsletter; publishing major texts and monographs; setting up advisory panels; pushing for legislation; recording archaeological sites; establishing an official membership; producing a comprehensive bibliography and data base on Aboriginal studies; and assisting in the setting up of carbondating facilities (Moser n.d.). The other major area where the Institute contributed to the development of Australian archaeology, was the way in which it, later, forced archaeologists to consider their political responsibilities to living Aboriginal people.

THE 'ABORIGINALIZATION' OF THE INSTITUTE

In the 1960s, the Institute was a primarily white academic institution and prehistorians, like the other researchers working in Aboriginal studies, carried out their work with little or no consultation with Aboriginal people. This situation changed dramatically in the early 1970s when Aboriginal people openly criticized the activities of the Institute. Throughout the decade, both the Institute and the discipline of Australian archaeology developed in a climate where Aboriginal people were politically active. It was in this context that an 'Eaglehawk and Crow' document was circulated to members of the Institute and other interested parties in 1974 (Widders, J., P.Thompson, B.Bellear & L.Watson 1974). This document can be seen as a turning-point in the history of the Institute

because it reflected the growing interest by Aboriginal people in AIAS affairs. Titled an 'Open letter concerning the Australian Institute of Aboriginal Studies' this five-page document details several issues of concern to Aboriginal people. While the focus of the discussion was the major international conference that was to be held at the Institute later in that year, the authors questioned the philosophy and foundations of the Institute. They argued that 'it has largely functioned as a fellowship of academics who supported each other to further their careers', where 'considerations about the lives and interests of the people they studied, or sincerely promoting any form of enlightenment in Australia have been, at best, secondary' (Widders, J., P.Thompson, B.Bellear & L.Watson 1974: 2). They questioned the relationship of the Institute to Aboriginal people since 1961, and claimed that the Institute had been resistant to introducing mild reforms that would enable Aboriginal people to become associate members of the Institute (associate membership was established to accommodate more junior scholars and non-academics), and that would see Aboriginal committees recommending lines of research (Widders, Thompson, Bellear & Watson 1974: 3). The two key issues at the heart of this document were whether the Institute should remain a strictly research institution that was concerned with traditional Aboriginal culture, and whether it should extend its brief to encompass contemporary Aboriginal matters or the social and political implications of the work that it sponsored. While there was some resistance to the idea that the Institute should become involved in what was seen as Aboriginal welfare (see response letters to Widders, Thompson, Bellear & Watson 1974), the Institute attempted to embrace the ideals set out in the document. In 1981, the Acting Principal Warwick Dix (1981:8) reflected on the history of the Institute and stated that the 'most significant development of all in the 1970s was the increased involvement of Aborigines within the Institute'.

It is important to note that some years before 'Eaglehawk and Crow', archaeologists had experienced Aboriginal resistance to their work. Mulvaney (1986:105) has documented how the first widespread Aboriginal opposition to archaeology, or more precisely fieldwork, surfaced in 1971 when he was acting principal of the AIAS. In response to concern about the publication of photographs of a secret-sacred ceremonial nature, Mulvaney organized a Canberra conference on the theme of 'field access'. He described this meeting as representing 'a faltering step towards the dialogue which followed during the next few years, particularly as encountered by the new AIAS Principal, Peter Ucko' (Mulvaney 1986:105). When Ucko became Principal of the Institute in 1972, he found a situation where Aboriginal people were calling for a role in the institutions that were responsible for their well-being. It was therefore not long before he became aware of the fact that there was no real or effective Aboriginal participation in the life and running of the Institute:

> I arrived there as a relatively cloistered academic, still sceptical about anything approaching 'action research' and with no hint of doubt about the

> sacred right of academic archaeologists and anthropologists to dig up, and into, whatever they pleased, regardless of whose lands, cultures or beliefs were being disrupted and overturned. I found that my Institute was a totally white institution -whites gave out money to whites, through white committees, to study the blacks. Very quickly this seemed to me to be an untenable situation and, as I met more and more Aborigines, whose interest and involvement in their own culture was at least as great as any academic's, I began to see, at first-hand, how a combination of European-derived committee structures, the English language, and so-called academic 'objectivity' could silence the voice of a whole population living in the same land but with a different cultural background.
>
> (Ucko 1987:2)

In his second year of running the Institute, Ucko introduced a policy retrospectively called 'Aboriginalization'. 'Aboriginalization' was a specific policy designed to foster Aboriginal participation in the affairs of the Institute, and more generally, in the field of Aboriginal studies. This policy was formally initiated in 1974, when the Council suspended certain Institute rules to enable Aboriginal people to become eligible as members of the AIAS (Ucko 1975:6), and when Aboriginal participation at conferences and on the Advisory Committees began. The precipitating factor behind formulating the Aboriginalization policy was the annual general meeting that was held at the Institute in 1974. Here Professor Bob Dixon (from the Linguistics panel) made a resolution to have a deliberate policy on Aboriginal involvement. In response to this event, a Committee of Enquiry was set up and a report with recommendations for implementing the policy was submitted to the Institute. The 'Aboriginalization' policy was developed in association with the issues in the 'Eaglehawk and Crow' letter, where specific concerns were raised about the lack of Aboriginal participation in the decision-making process.

Further developments in the 'Aboriginalization' of the Institute included the establishment of a new Aboriginal Advisory Committee in 1975. Among the first recommendations made by this Committee was that there be greater Aboriginal representation in the Membership and Advisory Committees, and that Aboriginal people be employed at the Institute (Ucko 1976:6–7). Decisions such as this led to the development of special programmes to train and employ Aboriginal people, both at the Institute, and in the various disciplines working in Aboriginal studies. Another result of the Aboriginalization policy was the introduction of Aboriginal-requested grants in 1976 which were, in practice, given primarily for the mapping of sites of significance and for the recording of rock art sites. Furthermore, after the 1976 Biennial General Meeting of the AIAS, a motion was passed that the Institute 'should continue as a body devoted to research in Aboriginal studies in which Aboriginal and non-Aboriginal students of Aboriginal society, work and jointly direct the policies, and that a central purpose of the Institute's work should be to gather, preserve and disseminate

information in ways useful to Aborigines, and in accordance with Aboriginal wishes' (Ucko 1977:7). It is clear that the desire to include Aboriginal people in the affairs of the Institute was a top priority throughout the 1970s.

The policy of 'Aboriginalizing', or promoting Aboriginal involvement in Aboriginal studies, had important implications for relations between Aboriginal people and archaeologists in the early seventies. As Murray & White (1981:261) have claimed, the main area of interaction between archaeologists and Aboriginal people was in the AIAS. They note how, during the 1970s under Ucko, it elected many Aboriginal members, moved into politically sensitive fields and funded research requested by Aboriginal people.

THE POLITICAL REALITY OF ABORIGINAL STUDIES

The Institute's concern to foster greater interaction between researchers and Aboriginal people had important implications for the development of Australian archaeology. The significance of the 'Aboriginalization' policy was that it made many archaeologists recognize their responsibilities to Aboriginal people. For instance, it was through many Institute initiatives that it became clear to archaeologists that their discipline had an obligation to the interests and concerns of Aboriginal people. One of the major achievements of the AIAS, with regard to archaeology and Aboriginal relations, was the way in which it situated prehistory in a contemporary political world.

Peter Ucko arrived in Australia towards the end of 1972, at a time when major political changes were taking place. Through acts such as setting up a Tent Embassy on the lawn in front of Parliament House in Canberra (January 1972), Aboriginal people made politicians take notice of their demands. They stressed the need for more effective participation in the institutions that governed Aboriginal affairs and that sponsored work in the growing field of Aboriginal studies. In 1972, the same year that Ucko was appointed as principal, the conservative government of twenty-six years was replaced by a new government with a radically different policy regarding the Aboriginal population. The conservative government had maintained the policy of assimilation of Aboriginal people into Australian society, whereas the new Labor government supported Aboriginal self-determination. A Commonwealth Department of Aboriginal Affairs and a multiplicity of Aboriginal organizations were set up to provide for the medical, legal, cultural, economic and housing needs of Aborigines and Torres Strait Islanders (Beckett 1988:204). The major development, however, was that the new Prime Minister, Gough Whitlam, responded to Aboriginal demands for land rights by initiating legislative changes. A new sense of hope was expressed by Aboriginal people, who were more optimistic that their demands would be heard by the new government (e.g., Gilbert 1978; Smith & Sykes 1981).

Under the new government expenditure on Aboriginal affairs doubled almost every year, from $AUS 23 million in 1971–2, to $AUS 141 million by 1975–6 (Broome 1982:181). The government gave an unprecedentedly high budget to the Institute, and research into Aboriginal studies was carried out at a vastly increased scale. This research was undertaken with a new political sensitivity toward the role of Aboriginal people in the production of knowledge about their cultural life. The most significant change was that the Institute enlarged its brief to include urban Aboriginal and current problems. In his first major report of the Institute's activities Ucko (1974:14) made a strong statement about the relationship between research and Aboriginal politics:

> What I have recorded in this Newsletter represents only a beginning. The Institute will have failed if, over the next year, it does not manage to place Aboriginal Studies in its rightful position within the world context of the study of human societies. We can only achieve this aim, a vital one for the understanding of the peaceful co-existence of different populations and social groups, if we adapt to the changing situation in Australia and if we can convince those in power that research and Aboriginal indigenous activity are intimately connected, and inextricably bound together.

Because the Institute became so politicized in the 1970s, archaeologists who had their research funded by the AIAS, and who attended AIAS meetings and conferences, could not avoid addressing Aboriginal political concerns. Professor Golson (then Professor of Prehistory in the Research School of Pacific Studies at the Australian National University) has commented that it was through the Institute that prehistorians got involved in and were exposed to the world of Aboriginal politics (Jack Golson pers. comm.). The importance of this was that at an early stage in its formation as a discipline, Australian archaeologists were addressing the impact of their research on Aboriginal people. As Ucko (1983:20) stated, 'Aborigines are forcing archaeologists to recognize that their discipline is one which sometimes can and does have (extreme) political and social consequences'.

At the first meeting of the association of Australian archaeologists at the Australian and New Zealand Association for the Advancement of Science in 1975, a number of archaeologists presented papers on the topic 'archaeologists and Aborigines'. That this theme was chosen indicates that it was seen as a vital issue. Furthermore, it is clear from the papers presented that Aboriginal people were already involved in archaeological projects being carried out in the state organizations, and that consultation with Aboriginal people was occurring at a range of levels. The papers, subsequently published in the official journal of the archaeological society (Creamer 1975; Golson 1975; Kelly 1975; Moore 1975; Stockton 1975; Sullivan 1975), reflect that there was a concern to address Aboriginal demands for control over their heritage. It was around this time that expressions of concern from Aboriginal communities in rural areas were voiced.

The Balranald people of New South Wales, for example, were concerned about archaeological investigations at Lake Mungo in 1974/1975. The statutory organizations that had been set up to manage the archaeological resource began to implement policies that promoted Aboriginal involvement and consultation. In the National Parks and Wildlife Service of New South Wales, consultation with Aboriginal people became part of the application procedure for permits to do archaeological work (Sullivan 1975). By the mid–1970s Aboriginal people were employed as site officers and field assistants, working within the state systems (e.g., Tom Kirk at the Victoria Archaeological Survey, and Ray Kelly at the National Parks and Wildlife Service, NSW). Some of these people have written about their training experiences in archaeology (Kelly 1975; Wilkes 1978), and some archaeologists have written about the implementation of training programmes for Aboriginal people in archaeology (e.g., H.Allen 1978). Developments such as these were related to policies set up by the AIAS to ensure that consultation with Aboriginal people became part of work practice. Before discussing the major areas where the AIAS fostered Aboriginal involvement in the practice of archaeology, it is important to outline two more specific ways in which the Institute served Aboriginal interests.

THE RETURN OF SKELETAL REMAINS

The Institute played a role in returning Aboriginal skeletal remains and in initiating negotiations between archaeologists and Aboriginal communities regarding such remains. For instance, in 1974 the Prehistory and Human Biology Advisory Committees of the AIAS joined together to write a letter to the Tasmanian government regarding the return of the skeletal remains of Truganini, an Aboriginal woman who died in 1876 (Ucko 1975:7). As Hubert (1989:46) has stated, 'this historic recommendation reversed the previous stance of only a few years earlier', and after pressure from the AIAS and the Aboriginal community, the Tasmanian government finally agreed that Truganini should be cremated. Furthermore, as early as 1976, the AIAS had taken the initiative in returning a skeleton to an Aboriginal community (Ucko 1977:20). Horton, who found the skeleton, believes this to have been the first such case in Australia (David Horton pers. comm.). Hubert (1989:50) also documents how the AIAS had to address Aboriginal opposition to the excavation of burial sites much earlier than other organizations and institutions in Australia. An important example of this was the delay in publication of Laila Haglund's excavation of 1965–1968 of the Broadbeach Aboriginal burial-ground in Queensland (Haglund 1976). These activities were important in the development of a specific archaeological policy regarding skeletal remains in the 1980s (see Meehan 1984).

At this time academic archaeologists started to discuss the implications of Aboriginal ownership of archaeological remains (Mulvaney 1979; Mulvaney 1981; J.Allen 1983; Frankel 1984; McBryde 1985; Sullivan 1985; McBryde 1986; Wright 1986; J.Allen 1987; H.Allen 1988; Bowdler 1988; Flood 1989;

Fourmile 1989). These discussions focused on the issue 'who owns the past?' and were a response to increased Aboriginal demands for access and control over their heritage. A major forum for this was the 1983 annual meeting of the Australian Archaeology Association in Hobart in 1983. Here Aboriginal people such as Langford (1983) forcefully presented their objections to the treatment of Aboriginal skeletal remains, and to the disturbance of Aboriginal sites through excavation. While most archaeologists responded positively to the Aboriginal demands for ownership and control over their heritage, and while a specific policy was established in support of the return and reburial of skeletal remains (Meehan 1984:128–33), a number of senior academic archaeologists expressed concern at the prospect of Aboriginal control (e.g., J.Allen 1983; J.Allen 1987; Mulvaney 1979; Mulvaney 1981). These scholars suggested that Aboriginal control might result in the cessation of archaeological activity, and asserted the value and benefits of archaeological research to Aboriginal communities. They argued that the Aboriginal past belonged to all Australians (Mulvaney 1981:20), and defined the notion of 'common heritage' on the grounds that as we proceed back in time, clear national or ethnic identities drop away from the data (J.Allen 1987: 8). As Lampert (1985:192) has argued, those who advocated the view that the past was public property maintained the doctrine of scholars belonging to colonial powers. At the same time however, Mulvaney (1986:104) argued that 'the dramatic change involved in the reassertion of Aboriginal cultural identity ranks as one of the most significant developments in Australian intellectual history, even though it proves inconvenient and challenging for field workers, and most Australians deride it as self-interested radicalism'.

LAND RIGHTS

When the Aboriginal Land Rights (Northern Territory) Act was passed in 1976, the AIAS began to play a major role in the lodging of claims for Aboriginal ownership of land. The Act allowed Aboriginal people to claim vacant Crown lands in the Northern Territory, provided that they could demonstrate their continuous occupation of and relationship to the land. This led the Aboriginal Northern Land Council and Central Land Council to seek the assistance of anthropologists and archaeologists in collecting the information they needed for their claims (see Berndt 1981; Peterson 1981). This became an essential activity carried out by the Institute for many years, with the Institute initiating a major project to monitor the social impact of uranium mining on Aboriginal communities in the Northern Territory (see Dix 1980:6). Ucko (1987:3) later claimed that

> it was self-evident to me that, at this point, the academics and scholars had no option but to leave the comfort of their cloistered walls, and enter the real world, to step away from their long-held academic priorities and to

> participate and help in the fight for the survival of the culture and heritage of the indigenous peoples….

The Institute appointed a full time land rights co-ordinator and research consultants to work on land rights. Ucko himself worked on Northern Territory land claims in 1979, and archaeologists and anthropologists were seconded from their jobs to work on such cases.[1] Meehan (1990) has detailed some aspects of her involvement in the Upper Daly land claim in 1980, and Berndt (1981) has discussed the involvement of professionals in compiling detailed evidence, in the form of maps and other data, in order to justify land claims (see also Cowlishaw 1983). Berndt also notes how the first direct anthropological statement relevant to land claims was prompted by a request made by the Interim Council of the AIAS as far back as 1964 (Berndt 1981:10). The Institute's commitment to land rights issues was also evidenced by the fact that a conference on the topic was organized. The Land Rights Workshop was held at the Institute in May 1979 and the archaeologists and anthropologists who attended would have become further aware of the political implications of their work.

WORKING TOGETHER IN THE FIELD: ABORIGINAL PEOPLE AND ARCHAEOLOGISTS ON SITE SURVEYS

A significant development in the history of interactions between archaeologists and Aboriginal people was Aboriginal involvement in site recording programmes. A key feature of the Institute's attempt to get Aboriginal people involved in Aboriginal studies was the emphasis it placed on providing training opportunities for Aboriginal people. To this end, Ucko announced the decision to commence training Aboriginal people within the Institute as well as encouraging their training at other institutions. It was primarily through the Aboriginal Advisory Committee that the employment and training of Aboriginal people was promoted (Ucko 1976:6–7). Not only were Aboriginal people trained to get technical skills (e.g., in photography), they were also trained in research work (e.g., in palaeoecology).

The Institute's focus on Aboriginal training had important implications for archaeology. One of the major areas where training was undertaken was in archaeological site recording. Site recording had been one of the major activities that the Institute sponsored throughout the 1960s. The concern with recording sites was central to the Institute's aim to collect information about Aboriginal traditional cultural life before it was lost or destroyed. The Institute constructed a register or national database of all known archaeological sites in the country, and set out to add as many sites to the register as possible (*AIAS Newsletter* 1965, 2: 20). The AIAS also began to fund site survey projects (e.g., the New South Wales North Coast Survey of 1970, *AIAS Newsletter* 1971, 3:18). Its background in supporting such projects led to the Institute being asked to run a large-scale site recording programme in 1972. When it decided to sponsor this national site

recording programme in 1973, the Institute made a significant contribution to the development of professional archaeology. In association with this programme Aboriginal people were trained as site officers, wardens and rangers, and archaeologists and Aboriginal people worked together at the state site protection authorities.

The organization of what became known as the 'Sites of Significance Recording Programme' was motivated by a conference on 'Aboriginal Antiquities in Australia', held at the Institute in 1972 and organized by Robert Edwards (later appointed as first Deputy Principal of the AIAS). Leading prehistorians such as Mulvaney pressed for government support in the recording of Aboriginal sites. Here the Minister for Aborigines, the Arts and Environment, Peter Howson, stressed the need for the location, mapping and protection of sacred sites all over Australia (*AIAS Newsletter* 1972, 3:24). The aim was to set up a programme that would delineate and protect areas of land, and assist state authorities in the development and administration of legislation in Aboriginal antiquities (Dix 1979:4). The Institute initially refused the government's offer but not long after his arrival Ucko reversed the Institute's previous decision, as part of the negotiations with the government for an increased Institute budget, and the creation of the new position of deputy principal. In 1973 funds were made available to implement a national site recording programme, and the Sites of Significance Recording Programme was set up (Ucko 1974:13).

The programme aimed to record all sites of traditional and historic importance to Aboriginal people. To carry out this task, a total of twenty-seven site recorders and trainees were sponsored by the AIAS and employed through the state authorities.[2] A 'Sites of Significance Advisory Committee' was established at the Institute to oversee and coordinate the programme.

The aims and priorities of the Sites of Significance Advisory Committee were ambitious and reveal that this committee took up an important role for directions in the discipline. They sought to coordinate a National Register of Sites; to recommend sites for the Australian Heritage Commission register; to act as a forum for the coordination and exchange of information on state sites programmes; to consider and recommend grant applications; to coordinate educational programmes regarding Aboriginal sites; to evaluate and monitor current site recording programmes; to give advice to the government regarding Aboriginal site recording; and to review and monitor the legislation regarding the protection of Aboriginal sites (Ucko 1979:14–15). The Sites of Significance Programme aimed to record a hundred sites per month. As sites were located, recommendations were made to the relevant authorities to ensure that they would be protected under the legislation (Ucko 1975:11). Closely related to the Programme was the 'Aboriginal Training Programme', where the Institute provided funds ($AUS 30,000 to each state) for the training of Aboriginal people. Dix reported that 'success has occurred through training projects in states that have, with Institute funding, provided training positions within state

organizations for twelve Aborigines, several of whom now have permanent positions within the state systems' (Dix 1979:6).

The wider objectives of the survey were outlined by Howard Creamer (1975: 18), who stated that 'before we could even begin to be effective in our work it was necessary for us to make the firm commitment that this research was to be first and foremost for the benefit of the Aboriginal people'. Furthermore, Aboriginal involvement was promoted on the basis that 'it is becoming increasingly obvious that any project that affects the Aboriginal people must involve them in a meaningful participation in both the research and the results right from the start' (Creamer 1975:21). It is clear from these statements that the programme constituted an important development in the way that archaeologists addressed issues of Aboriginal control. Creamer (1975:17) claimed that the survey had considerable social responsibilities, and that it actively sought to promote a revival of interest in Aboriginal culture.

The important point to make about the Sites of Significance Programme was that it changed its aims as a result of Aboriginal participation in the project. Archaeologists were forced to confront Aboriginal associations with the land and with specific sites. While the initial emphasis had been on locating archaeological sites or placing 'dots on the map', it soon became clear that Aboriginal people had strong views about the aims and function of the programme. For example, Kelly (1975:16) argued that:

> we have to make sure first that the knowledge is preserved in its Aboriginal meaning and then fed back into the people generally…. No doubt our bosses at the Institute of Aboriginal Studies think the knowledge has been well preserved, but I think that although they have the facts they do not have the true Aboriginal meaning. They are only preserving a white man's interpretation —about as good as our understanding of what it is to be an Eskimo. I see the job of people like us on the Survey of Aboriginal Sites to be that of collecting the meaning as well as the facts, and then trying to find a way to give that complete understanding to all our people.

As a result of getting such feedback, the Committee acknowledged the importance of returning information to the communities, and shifted its emphasis from visible 'traditional' and prehistoric sites, to sites that were sacred or significant to Aboriginal people themselves. As Layton (1992:27) has recently argued, Creamer and Kelly were notably successful in bringing the persistence of Aboriginal traditions in New South Wales to the attention of the state organizations. Traditional archaeological concepts of 'site' were being radically revised and contemporary living sites started to be recorded. The Institute started talking in terms of landscapes, in which there were interconnected sites of importance to living Aboriginal communities. The Sites of Significance Committee emphasized that the top priority was the recording of sites of significance to Aboriginal people and furthermore, that:

> The committee is particularly conscious of the fact that its concerns are directly those of Aboriginal people throughout Australia, arising from their aspirations and needs, and therefore recognises as its priorities those programmes and proposals which will encourage and enhance Aboriginal identification or re-identification with land and sites.
>
> (Ward 1979:v)

In the *Site Recorders Newsletter* it was outlined that the priority of the project was to record living archaeological sites, ethnographic sites and sites of significance to Aboriginal people. Ucko (1983:17) later remarked that 'in a complex set of manoeuvres, site recorders were pressurized by the AIAS into focussing on sites of significance to *living* Aborigines and to the inter-relationships of such sites'. Beyond recording sites of significance to Aboriginal people, the Sites of Significance Committee stated that the main function of the programme was to define sites and to advise on the protection of sites in terms of Aboriginal practice and concepts (Ucko 1977:9–10). McBryde has remarked that the Sites of Significance Committee provided a forum where Aboriginal people could say 'this is what we want people to work on' (Isabel McBryde pers. comm.).

The Sites of Significance Programme had important implications for academic archaeology in that it made archaeologists revise their tendency to define sites strictly in terms of being visible physical relics and artefacts. The Programme also led to a great deal of important site recording work being published in the literature (e.g., Creamer 1980; Creamer 1983), and led to more interaction between archaeologists and Aboriginal people. For instance, a site recorders' meeting was held in conjunction with a prehistory conference held by the Research School of Pacific Studies, Australian National University, at Kioloa in 1979. As Dix (1980:8) noted, 'the meeting provided an opportunity for site recorders and others, but Aborigines in particular, to meet people currently engaged in research into Australian prehistory'. Furthermore, archaeologists were exposed to Aboriginal opposition to some of their ideas and work.

Aboriginal training on other archaeological field projects was also sponsored by the Institute. For example, H.Allen (1978) was funded by the Institute to carry out an excavation and training programme in northern Australia in 1976. Allen (1978:23) emphasized the importance of securing Aboriginal people jobs after they had been trained, and argued that the employment of Aboriginal people as site recorders with state museums, national parks and archaeological services 'marks a really important change in black/white relations within the discipline'. Allen also discussed how working with Aboriginal people affected the goals of the field project, and led to the realization that archaeology was profoundly Eurocentric in its aims and interests.

WORKING TOGETHER ON THE MACHINERY OF GOVERNMENT: ARCHAEOLOGISTS AND ABORIGINAL PEOPLE ON THE ADVISORY COMMITTEES

Another important result of the 'Aboriginalization' policy was that archaeologists began to work with Aboriginal people on committees. Soon after taking up his position as principal, Ucko made important changes to the advisory panel system that operated under the former principal, Fred McCarthy. The original advisory panels that were set up for the major disciplines working in Aboriginal studies in the 1960s (human biology, material culture, social anthropology, prehistory, linguistics), were totally transformed by Ucko into what Hiatt has described as 'powerful academic units' (Les Hiatt pers. comm.). The advisory committees met twice a year, at the Institute's expense, with one meeting set aside to discuss grant applications, and the other to discuss the nature and future development of each discipline. Prior to this, the panels mainly discussed grants, and decisions were made by correspondence. Ucko (1975:7) claimed that the committee meetings were concerned not only with considering grant applications and proposing urgent research priorities, but also with 'policy and theoretical discussions regarding the nature of their respective academic disciplines'. An Aboriginal 'social issues adviser' was later appointed to each advisory committee to define Aboriginal interests that could be advanced by the Institute, to formulate specific proposals for research, and to advise on matters (Ucko 1977:11). Furthermore, any member of the Aboriginal Advisory Committee (set up in 1975) had the right to attend meetings of any disciplinary advisory committee. Archaeologists and Aboriginal people participated in the Prehistory Committee's work of assessing grant applications, making recommendations on future research, and discussing disciplinary issues.

The other committees that dealt with archaeological and heritage issues included the Material Culture Committee, the Sites of Significance Committee and the Human Biology Committee. The meetings for all these committees were an important forum for debate in the development of Australian archaeology, particularly in that they provided the opportunity for a small community of scholars, who were now able to meet more regularly and more frequently to discuss disciplinary issues on a regular basis. Also important were the interactions that archaeologists had with linguists and others who worked closely with Aboriginal communities.

Besides being active on the disciplinary committees, Aboriginal people worked on other committees that were set up to ensure greater Aboriginal representation in the membership of the Institute and in the other advisory committees (see Ucko 1976:6–7). As early as 1974 the Prehistory Committee was informed that the Institute was setting up a special committee for Aboriginal-requested research. In 1976, the Research and Membership Committee of the AIAS recommended that a new committee be set up to 'assist expressed

Aboriginal interests in the machinery of governing and in the development of their participation in social, political and legal institutions (Ucko 1977:10). In his review of Institute activities for 1978, Ucko (1979:6) highlighted the marked increase in the involvement of Aboriginal people in Institute activities at all levels of its operations. Aboriginal participation in the advisory and other Institute committees was perhaps the most important feature of this development.

PROTECTING THE RESOURCE: THE DEVELOPMENT OF LEGISLATION TO CONSERVE ABORIGINAL SITES

Central to the 'Aboriginalization' of archaeology was the implementation of protective legislation for Aboriginal sites (see Ward 1985). One of the main activities undertaken by the AIAS in the 1960s was the push for the establishment of protective legislation for Aboriginal antiquities. From the outset the Interim Council of the Institute was 'in correspondence with all State Governments in an attempt to elicit information about laws and policies' regarding Aboriginal sites (*AIAS Newsletter* 1963, 1:5). At the Institute's first Biennial Meeting in 1964, it was stated that the protection and preservation of Aboriginal relics was a major concern (*AIAS Newsletter* 1965, 2:38). Investigations into methods of European preservation were sponsored in order to develop legislation for Aboriginal antiquities (*AIAS Newsletter* 1970, 3:32), and the principal, who had long been concerned with the setting up of legislation to protect sites (e.g., McCarthy 1938), was very supportive of the initiatives undertaken in this area by the professional archaeologists.

Archaeologists utilized the resources of the Institute to sponsor conferences and publications on the protection of archaeological sites. Two major conferences were organized that dealt exclusively with the topic. One conference titled 'Aboriginal Antiquities in Australia', was held in 1968, and the other, titled 'The Protection of Australia's Aboriginal Heritage', was held in 1972. The proceedings of both conferences were published by the Institute (McCarthy 1970; Edwards 1975).

Flood (1989:80) emphasizes the centring of the legislation around protecting visible 'relics'. She outlines how the early legislation was archaeologically biased, because the motivation behind it was to protect visible relics from amateur research, collectors and development pressures. She claims that 'characteristic of the legislation of this period was the focus on archaeological sites and the neglect of sites which contain no material evidence of Aboriginal occupation, but which are sacred or significant to Aborigines'. By working together on the site survey programme archaeologists began to address the issue that the most significant Aboriginal sites were not necessarily 'dead' sites from pre-contact times. Often the most significant sites to Aboriginal people were natural features of mythological significance, and ceremonial and burial sites which for

Aboriginal people were not prehistoric but were contemporary sites (Creamer 1975:22). This involvement ensured the revision of major assumptions which archaeologists maintained in developing protective legislation for Aboriginal heritage. For instance, the emphasis on protecting visible prehistoric relics was shifted and contemporary sites that Aboriginal people declared to be significant were included. As a result archaeologists started to develop a different attitude to which sites should be protected and to their categories for assessing archaeological significance. The Sites of Significance Committee recommended to the government's Committee of Inquiry into the National Estate that the government should introduce legislation for the uniform protection of Aboriginal sites of significance. They also recommended that the provisions of legislation be drawn up in discussion with the state authorities, who were conserving and protecting sites, and that Aboriginal people were to be consulted throughout the drafting of the legislation (Ucko 1975:12).

S.Sullivan (1975:28), head of the archaeology section at the National Parks and Wildlife Service, noted that the New South Wales legislation of 1970 made no mention of Aboriginal people. By 1975, as a result of Aboriginal involvement in site survey work, the situation had changed. Sullivan notes the change in excavation permit forms, which now required Aboriginal consultation and an agreement that Aboriginal people should be employed, in certain cases, to protect their own sites. She outlines how Aboriginal participation led to the revision of criteria by which sites were assessed. For instance, the distinction between sites that were significant according to traditional cultural values, and sites that archaeologists regarded as technically prehistoric was acknowledged as being a white (European) distinction. Ucko (1983:15) claimed that 'individual archaeologists were required, by the Institute's new policy of involving Aborigines in the decision making processes, to discuss their aims and methods with Aboriginal people who lived near the sites and areas they wished to examine'. By this time the National Parks and Wildlife Service had formed an Advisory Committee on Aboriginal sites (see Sullivan 1983), and as a result almost all states modified their outlook on Aboriginal relics. In this sense initiatives by the Institute and the Parks Service brought Aboriginal people and archaeologists together to discuss issues concerning the development of legislation.

The Institute also played an important role in the setting up of protective legislation for Aboriginal heritage, in that it was consulted by the various state authorities. For example, the Institute played a key role in the drafting of the Northern Territory legislation in the late 1970s. Specifically, they added a section to the legislation on sites and areas of living significance, and introduced a special category that dealt with sacred sites.

MAKING CONTACT: ARCHAEOLOGISTS AND ABORIGINAL PEOPLE AT THE INSTITUTE

One of the ways that the Institute fostered communication between archaeologists and Aboriginal people was through its efforts to involve Aboriginal people in the various activities of the AIAS, particularly in the meetings and conferences. It was at these and many other occasions, that the Institute provided archaeologists with the opportunity to meet and get to know Aboriginal people. The AIAS General Meetings and sectional conferences lasted for three to four days at a time and involved long days of discussion and conversation that continued well into the night (Jack Golson, Les Hiatt, Rhys Jones, Betty Meehan pers. comms). At these meetings, researchers and Aboriginal community members not only discussed issues concerning research and the management of Aboriginal heritage, they spent time socializing and getting to know each other.

Jones (1985b) has documented an important example of how the AIAS facilitated the getting together of archaeologists and Aboriginal people. He talks about how an Aboriginal man, Frank Gurrmanamana, who was elected as a full member of the Institute, spent time with him and Betty Meehan in Canberra when he came down for an AIAS meeting. The effects of such experiences were important in developing an archaeological perspective in various issues. For instance, Jones describes the visit in relation to a discussion on Aboriginal land use, and concepts of space and boundaries. He describes the response of Gurrmanamana to the city of Canberra, which to him with 'its geometric streets, and the paddocks of the six-wire fences were places not of domesticated order, but rather a wilderness of primordial chaos' (Jones 1985b:207). This is just one example of many, where Aboriginal people were resident in Canberra and got to interact with scholars and other Aborigines from other parts of the country.

Another, different kind of example of how the AIAS facilitated interaction between archaeologists and Aboriginal people, was at major academic events such as the 1974 International Conference held by the Institute. While few Aboriginal people attended this conference, those that did raised the issue of white academic control over the field of Aboriginal studies. For instance, it was at this conference that the issues raised in 'Eaglehawk and Crow' were addressed. This was also an important event in view of the fact that many overseas scholars attended the conference and commented on what a unique event it was (see for example comments by G.Isaac, W.W.Howells, L.Binford and A.Marshack (Ucko 1975:12–13)).

Furthermore, when archaeologists visited the Institute they would often meet Aboriginal people who were employed or being trained there (e.g., the assistant palaeoecologist, Richard Wright, who was appointed in 1977). Archaeologists would also meet Aboriginal people who were visiting the Institute or doing research there. When the AIAS took Aboriginal community members to other locations where heritage material was held (e.g., the Thomson collection held in

Melbourne), communication between archaeologists and Aboriginal people was fostered. Besides conferences, meetings and working situations, the Institute also organized social events where archaeologists and Aboriginal people would meet.

THE 'ABORIGINALIZATION' OF AUSTRALIAN ARCHAEOLOGY: CHANGING RESEARCH DIRECTIONS

The AIAS's response to the demands made for increased Aboriginal involvement in the field of Aboriginal studies affected Australian archaeology in several major ways. Aboriginal demands for control of, and access to, their cultural heritage led to major changes in archaeological practice, and this, in turn, led to changes in research questions and interests. The most immediate transformation was related to the Institute's shift in interest from traditional Aboriginal life to contemporary Aboriginal life. In 1974 Ucko (1974:8) argued that 'if we are to keep pace with the changing political and social situations of the Aborigines, the Institute can no longer afford to deal only with traditional Aboriginal life'. The conception of prehistory as a discipline that dealt with 'relics' had to be radically changed. With the Sites of Significance Programme, archaeological sites took on a completely different meaning when examined in the light of Aboriginal interests. Aboriginal people had led archaeologists to reconsider their conceptions of what constituted a 'site', and this led to a greater archaeological interest in how Aboriginal people viewed the landscape.

It was also through the advisory committees that changes in the definition of the discipline could be seen. Ucko (1983) has discussed how the initial research aims of the Prehistory Committee were very traditional. There was an emphasis on questions concerning the antiquity of human occupation and the racial affinities of Aboriginal people. These research aims substantially changed in the 1970s when committee members emphasised an interest in human-land relationships. As Willmot (1982:4) has argued,

> In the initial phase the Institute's activities seemed to concentrate on two important activities. The first was to bring into focus Aboriginal Studies in Australia; the second was to search into Australia's distant past and to substantiate the long and important Aboriginal occupation of this continent. The second phase altered the emphasis of Aboriginal Studies, bringing them closer to the real cultural life of the Aboriginal people.

The work of this period was instrumental in demonstrating the depth and legitimacy of the cultural base of Aboriginal society.

The committee meetings were important in the development of Australian prehistory in the sense that they brought together members of the profession who were geographically isolated from each other, and who rarely met to discuss research initiatives. The meetings also brought together researchers who would

take their decisions back into the training (university) environment. While many of the other advisory committees spent much of their time assessing grant applications, the Prehistory Committee was noted for spending much of its time discussing research issues. For instance, the Committee emphasized that 'its discipline should be accepted as including the ecological studies of living Aboriginal groups' (Ucko 1974:7).

The way in which Aboriginal people made use of and reinterpreted archaeological findings was another dimension of how Aboriginal involvement affected the discipline. The fact that Aboriginal people presented their own interpretations of archaeological sites meant that archaeologists were forced to confront how the past was used by others, and that the past was being reinterpreted by people who had claims to know more than they did. Archaeologists began to address the role their knowledge played in the construction of contemporary Aboriginal identity. On one level, archaeological knowledge was incorporated into such constructions (for instance, the 40,000-year-old date for Aboriginal occupation of the continent). On another level, a central part of defining Aboriginal identity was the rejection of white scholarship, in particular anthropological and archaeological research (see Langford 1983). The contributors in a recent volume on Aboriginal identity (Attwood & Arnold 1992), argue that the resistance to interpretations made in white scholarship is just as important as the acceptance and use of such interpretations.

During the 1980s there were a number of important discussions about the prospects of an archaeology with Aboriginal involvement, notably those by H.Allen (1981), Bowdler (1983), Creamer (1983; 1990), S.Sullivan (1985), McBryde (1988;1989). Sullivan (1985:151) outlined the centrality of Aboriginal involvement to the practice of archaeology, and stated that 'there is considerable evidence that increasing Aboriginal involvement and control will have very exciting and beneficial results for the general community'. Bowdler (1983) encouraged more Aboriginal involvement in archaeology, which she argued would result in the discipline becoming more dynamic, exciting and challenging. Discussions about the implementation of training programmes for Aboriginal people, and the process of consulting with Aboriginal communities, appeared in the literature (Creamer 1983; H.Sullivan 1984; Buchan 1985; Jones 1985a; Lewis & Rose 1985; Beck & McConnell 1986). Jones (1985:19) reported on the negotiations with Aboriginal communities at Kakadu, believing them to be among the most detailed and informative dialogues carried out between archaeologists, government officials and Aboriginal people. In 1983 an important workshop on site management at Kakadu was held, with nearly 50 per cent Aboriginal involvement (H.Sullivan 1984). Despite these works, it was not until more recently that the positive effects of Aboriginal involvement became more fully realized.

In the 1990s the debate on interactions between archaeologists and Aboriginal people has shifted to recognition of the value of Aboriginal involvement and the 'building of bridges' (Meehan 1990; Pardoe 1990; Pardoe 1991a; Pardoe 1991b;

Pardoe 1992; McBryde 1992; Tacon n.d.; Williams n.d.). Many points have been made that highlight the increasing recognition of the impact of Aboriginal involvement on the discipline. The process of building bridges between archaeologists and Aboriginal people has been discussed by Meehan (1990), who shows how this process has been going on for some time, as is evidenced in the early work on Aboriginal land claims. While the question of Aboriginal demands to rebury remains of their ancestors still remains a contentious issue (e.g., for Mulvaney 1991), Australian archaeologists, in general, accept the decision to rebury Aboriginal skeletal remains. Pardoe (1985; 1990; 1991a; 1991b; 1992) has written extensively about how Aboriginal involvement has affected his work in physical anthropology. He argues that '[T]he study of ancient human skeletal remains cogently demonstrates the social arena in which science is situated' and that this arena has 'in the last decade, undergone radical change' (Pardoe 1992: 132). McBryde (1992) has discussed the significance of the development of a code of ethics that specifically acknowledges ownership of cultural heritage by the descendants of its creators. Williams (n.d.) discusses how demands made by Aboriginal people led to the introduction of new guidelines in state organizations. She makes the important point that while this did lead to disruption to some areas of archaeological activity, it became clear that Aboriginal control did not mean the end of archaeology. Rather, argues Williams, the real issue was one of 'asking first' and demonstrating respect for the particular Aboriginal communities involved. Tacon (n.d.) has also outlined some of the significant developments in the strengthening of the bridge between indigenous and non-indigenous interests in the past. He emphasizes the return, by museums, of cultural material to indigenous peoples where, in the process, much attention has been devoted to 'working together'.

Flood (1989:83) in her discussion of the construction of legislation argues that the '1970s revealed an additional skill needed by Australian prehistorians: the ability to communicate effectively with Aborigines'. She claims that the relationship between archaeologists and Aboriginal people has changed and 'now tends to be more of a cooperative venture between archaeologists and Aborigines' (Flood 1989:85). In a discussion of the debate 'Who owns the past?' in Australia, Murray (1993) highlights the legislative recognition of Aboriginal interests. In another paper that looks at the changing discourse of Australian archaeology, Murray (1992:13) suggests that the Aboriginal interest in heritage appears a likely source of methodological and conceptual innovation in Australian prehistory. One of the most recent publications that deals with relations between Aboriginal people and archaeologists (Birckhead, de Lacey & Smith 1992), like the Kakadu workshop papers (H.Sullivan 1984), provides many examples of how Aboriginal people have contributed to and challenged the wider area of heritage management.

One of the most exciting things to have occurred in this context is the development of 'community-based archaeology'. Greer (Greer & Fuary 1987; Greer 1989) and Clarke (1993), for instance, have carried out field projects based

on the concept of 'community archaeology', which goes beyond simply negotiating with Aboriginal organizations for permission to carry out field research and employing Aboriginal people as assistants in fieldwork. In such projects Aboriginal people do not simply act as participants and advisors, they work with the archaeologist in framing the research questions of the project (Shelley Greer pers. comm.). As Clarke (1993:12) has also recently stated, community archaeology takes the 'basic process of consultation one step further and directly involves community members in the design and execution of the field research.'

CONCLUSIONS

In the 1950s and 1960s archaeologists had developed an intellectual framework for dealing with Aboriginal prehistory with little input from living Aboriginal people. Through its relationship to the AIAS in the 1970s the discipline of Australian archaeology started to take into account Aboriginal interests, and was significantly affected as a result. Beyond fostering Aboriginal involvement in the practice of archaeology, the AIAS ensured that archaeologists became aware of the political dimension of their field. Furthermore, it was largely through the initiatives of the Institute in the 1970s that the research agenda of Australian archaeology began to incorporate Aboriginal perspectives. Since then, the concerns of Aboriginal Australians have been central to the development of the discipline.

The relationship between archaeologists and Aboriginal people in Australia has a long and complex history. In looking at the professionalization of Australian archaeology in the 1960s and early 1970s, it has become clear that Aboriginal involvement occurred at an early stage in the formation of the discipline. It is also clear that archaeologists engaged with issues concerning the Aboriginal political struggle when they were still in the process of defining their discipline. This is important in that the development of an academic discipline of archaeology in Australia was closely tied to the development of Aboriginal studies as a field in its own right. One of the central points in this chapter has been that there is a history of 'working relationships' between archaeologists and Aboriginal people that predates the more recent and contentious academic debates about the ownership and control of Aboriginal heritage. Furthermore, the establishment of these working relationships was inspired by the demands made by Aboriginal people to participate in the process of making decisions about their cultural heritage. While consultation with Aboriginal people has long been part of the procedure for acquiring permits for archaeological surveys and excavations (see Sullivan 1975), and while recognition of Aboriginal ownership was formalized in a code of ethics created by Australia's association of archaeologists (see J.Allen 1983 for initial resolutions concerning ownership of archaeological resources, and Davidson 1991), archaeologists have a history of working with Aboriginal people in state and federal organizations, and in the

field. Of particular significance was the role played by the National Parks and Wildlife Service in the state of New South Wales. In looking at where, when and how an Aboriginal transformation of Australian archaeology might have taken place, it has been necessary to go back to the early 1970s, when state organizations began to employ Aboriginal people to work in the heritage area, and to examine how archaeologists were confronted with the political dimension of their profession.

Towards the end of the 1970s the close relationship between the AIAS and the discipline of Australian archaeology began to shift. There were a number of reasons for this. First, prehistory was by far a more independent and self-sufficient discipline than it had been in the 1960s and early 1970s. It now had its own institutional infrastructure, and was not so dependent on the resources the Institute had provided in areas of funding, publishing and the organization of conferences. The other significant factor was that the wider discipline of Aboriginal studies had significantly changed in its aims and priorities (see Willmot 1983, Willmot 1985). Other neglected disciplines such as Aboriginal history (which dealt with the history of Aboriginal communities in the contact period), were getting the same kind of support from which archaeology had benefited. The changes which occurred at the Institute during the 1970s, particularly in the committee system, the membership prerequisites and the research priorities, reflected the expansion and diversification of Aboriginal studies.

Most of the discussion on the topic commonly referred to as ‘Aborigines and archaeologists’ has focused on the controversial nature of interactions between Aboriginal people and archaeologists, in particular the issue of the treatment of skeletal remains. This chapter has sought to add another dimension to the debate, by documenting the involvement of Aboriginal people in the practice of archaeology. Aboriginal responses to archaeology have tended to be discussed mainly in terms of academic and Aboriginal interactions. Very little has been written about the role of the federal and state organizations in facilitating the involvement of Aboriginal people in Australian archaeology and the social context or the role of Aboriginal political activism in the 1970s. For these reasons I have focused on the activities of the AIAS in the 1970s, since it was one of the main organizations that sought to incorporate Aboriginal perspectives and make them central to its operations. In particular I have focused on how the Institute’s policy of ‘Aboriginalization’, introduced in 1974, led to the involvement of Aboriginal people in several major areas of archaeological activity. My argument is that the Aboriginal transformation of archaeology began at an early stage in the history of the profession in Australia, and that the Institute played an important role in facilitating this transformation. The Institute responded positively to Aboriginal demands for involvement in the study of their cultural heritage. Above all, it sponsored the development of working relationships between archaeologists and Aboriginal people that have subsequently become a central component of the discipline’s identity.

NOTES

1 Myrna Tonkinson was appointed to the first of these positions and Robert Layton and Nancy Williams, amongst others, to research consultantships. Those seconded from their jobs included Betty Meehan, Peter Sutton and John von Sturmer.

2 The AIAS provided assistance to the following state authorities to assemble survey teams: the National Parks and Wildlife Service in New South Wales, the Department of Aboriginal and Islander Advancement in Queensland, Queensland University, the Archaeology and Aboriginal Relics Office in Victoria, the Aboriginal Material Culture Commission in Western Australia, the Aboriginal and Historic Relics Administration in South Australia, the National Parks and Wildlife Service in Tasmania, the West Australian Museum, and the Museums and Art Galleries of the Northern Territory.

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CHAPTER EIGHT
PREHISTORY IN A MULTICULTURAL STATE
A commentary on the development of Canadian archaeology

QUENTIN MACKIE

INTRODUCTION

In 1931 D.Jenness, the noted Canadian anthropologist, wrote that Canadian archaeology is 'a child of recent years that has not yet reached full stature' (Jenness, in Noble 1973:49). Some years later Noble, recipient of one of the first Canadian doctorates in archaeology, added 'it is now fair to say that archaeology in Canada has attained a young adult stature in most of the Dominion's ten provinces' (Noble 1973:49). The chief of the Archaeological Survey of Canada subsequently turned the thought in an unexpected direction:

> In my opinion the analogy is not completely inappropriate, because the growth in question has been anything but normal. Indeed, to stay with the analogy would be to observe a child who exhibits little or no growth over many years and then suddenly explodes into a hulk possessing both Dr Jekyll and Mr Hyde characteristics.
>
> (Wright 1985:421)

In Wright's characterization, it is a traditional culture-historical approach which plays Dr Jekyll to a Binfordian Mr Hyde. Continuing, with some apprehension, this traditional train of thought, it can be said that the Jekyll and Hyde personae of the mature discipline in Canada are now experiencing a late-developing 'sibling rivalry' as other voices, notably from the First Nations, claim an interest in the traditional activities of archaeologists. Issues such as the reburial of human remains and the repatriation of material culture from museums, the use of archaeological interpretation in land disputes, and the feminist critique of the discipline have thrown into 'sharp relief' (Wylie 1993:11) the social and political agenda of archaeology.

Fortunately, there is evidence that Canadian archaeologists are starting to realize the inherently political qualities of their work (e.g., McGhee 1989). But, before examining this 'sibling rivalry', it is necessary to understand something of the family genealogy. This chapter discusses the development of archaeology in Canada, with special reference to foreign influences. Two regions—Quebec and

the British Columbian coast—are examined in some detail in the expectation that their archaeology might have been exposed to identifiable European influences. Finally, future directions in Canadian archaeology are considered, especially the relationship between archaeology and the Native communities.

OVERVIEW OF CANADIAN ARCHAEOLOGY

There has been surprisingly little overt discussion of Canadian archaeological theory. Short historical reviews such as those by Noble (1973), MacDonald (1977, 1982), and Forbis & Noble (1985) tend to be lists of excavated sites and programmes for future work, usually emphasizing the vast unknown rather than current theoretical orientation. Neither Wylie nor Trigger, perhaps the most likely Canadian candidates, have shown much interest in Canadian theory as a discrete subject, while other leading Canadian archaeologists are only now reaching a reflective age. Furthermore, as discussed below, Canadian identity is quietly subsumed within studies which use the casual and ambiguous term 'American' as if it were synonymous with North American.

The most explicit statement of the character of Canadian archaeology is by Wright (1985:422, 429). Within this avowedly opinionated article, a diagram schematically represents the 'major values' bearing on archaeology in Canada. The most notable feature of this is a terminal side-branch labelled 'Binfordian Positivism', which culminates in the terse epitaph 'Runs Course'. The prospect for the 'main drift of Canadian archaeology and its resources' is brighter, if vague: an 'expanding comprehension of humanity through time and in space and in relationship to its environment' is predicted to occur.

In effect, Wright expounds a vision of Canadian archaeology as possessing a privileged ecological immediacy. He suggests (Wright 1985:425–8) that a non-doctrinaire ecological approach is what distinguishes Canada within Americanist archaeology. Not only can archaeologists, while doing their everyday fieldwork somewhere in the ubiquitous Canadian boondocks, 'still often talk to native hunters about hunting and other relevant topics' but they can also 'observe the unmolested interplay of the caribou and the wolf, the barrenground grizzly and the ground squirrel, as well as rivers teeming with char…' (Wright 1985:425–6). While praising the historicist tendencies in Canadian archaeology, Wright places Native Canadians in a timeless ecological niche, suggesting that 'these two interrelated factors of relatively pristine biology and native cultures…probably account for the tenacity with which many Canadian archaeologists adhere to traditional anthropology' (Wright 1985:426).[1]

Yet, what is this traditional anthropology? For the Canadian archaeologist it may be a time when Natives were seen as

> part of nature, and as such, to be studied as natural history rather than as art. As products of nature, they served, like their analogs in geology and

> palaeontology, as the keys to unlock the gates to the long unrecorded history of humanity.
>
> (Gruber 1986:170)

Archaeology and anthropology in Canada have a long record of institutional association with natural history. For example, Sapir headed the Anthropology section of the Geological Survey of Canada (GSC) during the early part of the century. For many years GSC publications were the premier channels of communication. Early issues of the GSC annual report find archaeology nestled in among geology, climatology and zoology. Other interdisciplinary natural history journals, such as *Syesis* in British Columbia, were important until the 1980s. Wright laments (Wright 1985:431) the loss of the naturalist's scope.

Until fairly recently, then, most Canadian archaeology could be fairly characterized as ecologically informed culture-history, carried out under the umbrellas of anthropology and natural history.

AMERICAN INFLUENCES ON CANADIAN ARCHAEOLOGY

There are three basic ways in which Canadian archaeology has been influenced by the United States: geographical and cultural proximity, academic training of the current senior generation of Canadian archaeologists, and the embedding of archaeology within Boasian anthropology.

Geographical and cultural proximity

Canada is unusual in having only one neighbour country, the United States. This international border is long, open, and separates two nations with similar academic traditions and, for the most part, a common language. Much of the Canadian population is clustered within a strip a few hundred kilometres wide along the border. The larger nation can dominate any such relationship by size alone. As the United States has ten times the population of Canada, its quantitative dominance in most academic endeavours, including archaeology, is no surprise. Furthermore, the opportunity for qualitative distinction is often lost because many authors lump Canada and the United States together when discussing 'Americanist' or 'Anglo-American' archaeology. Embree (1989:63), for example, expends only three words to justify the inclusion of Canada within his category 'American Theoretical Archaeology'. Another example is a review volume of North American archaeology (Fitting 1973) in which some account is taken of modern political boundaries. Still, the chapter entitled 'Canada' covers only a rump portion of the country, stripped of its western coast and Arctic expanses. Canadian content creeps into four other chapters, including one entitled 'Northeastern United States'.[2]

Similarly, Trigger, who as a Canadian might be expected to make the distinction, includes (Trigger 1989a:271) work done in Ontario by the Canadian archaeologist Wintemberg within a section of *A History of Archaeological Thought* entitled 'Early Functionalism in the United States'.[3] The blurring of Canadian and American archaeology is exacerbated by the long-standing practice of delimiting archaeological regions on the basis of cultural and natural areas, such as those defined by Kroeber (1939). Few, if any, of these coincide with political boundaries, past or present.

Academic training

Canadian universities expanded rapidly during the 1960s and 1970s to accommodate the 'baby-boom' population bulge, and to meet a generally increasing interest in higher education. Between 1966 and 1969 alone the number of anthropologists at advanced degree-granting institutions jumped from 68 to 116 (Kelley & Hill 1991:196). The first Canadian doctorates in archaeology were not granted until 1967 and 1968, and the supply was meagre until the mid–1970s. Prior to this, there was no choice but to hire foreign-trained archaeologists to fill the new university posts.[4] Most founded or solidified professional archaeology at their respective universities. There were also some American-trained colleagues in museums and other institutions, who may have also have taught at local universities.[5] This small group of scholars formed the core of the archaeological community during the 1960s and 1970s, and had a decisive influence on the direction of Canadian academic archaeology. In this respect, the absence of individuals with European degrees is striking.[6] Indeed, William Taylor (Ph.D. Michigan), former director of the National Museum of Man, has openly called for less influence from the United States, and more from elsewhere (Taylor 1977:154). Why this did not occur naturally is a valid question. It would, for example, be interesting to understand why Canada did not share the fate of Australia, which was colonized by Cambridge archaeologists (Murray & White 1982; Moser 1995).

After it became possible during the late 1960s to study for a Ph.D. within Canada, the subsequent 'generation' includes students of the American-trained vanguard.[7] However, a steady stream of degrees continued to flow from the United States.[8] A much smaller number gained doctorates in Europe.[9]

Fifty-five per cent of the archaeologists active in 1977, near the end of the expansionary period, had degrees from American institutions. Over half of the remainder gained their Ph.D. at the University of Calgary (Wright 1977:2), while only two had graduated from universities in Europe. It is clear, therefore, that professional archaeologists active in Canada were, with very few exceptions, trained either in the United States or in Canadian departments under the supervision of American-trained scholars. Under these circumstances, European influence was related to an individual scholar's interests and commitment, and not part of any systematic process.

Boasian anthropology

Perhaps the single most influential American tradition adopted by Canadian universities is the institutionalization of archaeology as a subdiscipline within holistic, 'Boasian', departments of anthropology. These anthropology departments, the first of which was established in the late 1930s at Toronto, are themselves dominated at the senior levels by American-trained social-cultural anthropologists.[10] Many were, or are, omnibus 'Departments of Sociology and Anthropology', placing archaeology even more firmly into a social-scientific environment.

However, the two largest concentrations of archaeologists—and the most productive training centres—have been at Simon Fraser University (Vancouver) (SFU) and the University of Calgary. Exceptionally, both are independent departments of archaeology, but not as a result of direct European influence. Richard Forbis (in press) describes how the Calgary department was founded in 1964 when the fledgling university was looking for

> 'innovative programmes' in an attempt to fill academic gaps left unclaimed by old established schools. Archaeology was one discipline with no secure home. [The new department] would no longer see archaeology as a handmaiden to anthropology.

Similarly, Carlson, co-founder in 1971 of the Simon Fraser University archaeology department, has commented (Carlson 1977:13) that a drift from ethnography to sociology in Canadian anthropology departments isolated and hampered both the teaching and practice of archaeology. The SFU department countered this by enabling the integrated teaching of archaeology with courses in natural history. Apart from academic empire-building, the prime motivation for the establishment of these separate departments appears to have been the widely held opinion in Canadian archaeology that an interdisciplinary academic environment was needed (Carlson 1977; Forbis in press).[11] Forbis (1977:11) had earlier disavowed his 'sentimental' attachment to anthropology as a 'holistic science', through the realization that anthropology was only a part, albeit a large part, of the archaeological universe.

This apparent distrust of anthropology is related to Trigger's (1984, 1989b) thoughts on the anti-historical (functionalist) tendencies in anthropology. The consequence, when combined with the generalizing spirit of the 'New Archaeology', and its systemic-ecological approach, was the abandonment of truly historical explanations. In this way an artificial and alienating split was opened between indigenous groups and their prehistory which

> suggests that native peoples do not possess real history and that Indians and whites have little in common. In Africa and elsewhere in the Third World similar attitudes have led to anthropology being fiercely rejected

> and criticized. In these countries, archaeology survives by associating itself with history.
>
> (Trigger 1977:10)

In effect, then, not only are members of the living culture alienated from their own history, but archaeologists are alienated from the very peoples whose past they study (Trigger 1977:10; see also McGuire 1992:241–3). Furthermore, the indigenous cultures become discouraged from actively participating in archaeology (Trigger 1986). Trigger (1984:306–7) concludes that the 'New Archaeology' was the manifestation of post-1945 American imperialism, and unwittingly carried out a programme that denied both national and indigenous traditions. Canada and Canadian First Nations were vulnerable to this for the same general reasons of American influence discussed above.

Trigger is an enigmatic figure in Canadian archaeology. Undoubtedly the country's most distinguished archaeologist, for years he has swum against the tide of mainstream archaeology by championing an approach respectful of the long-term history of colonized indigenous peoples. He (Trigger 1989a:xiv) notes the influence of Gordon Childe on his attempts to reconcile a materialistic approach with historical diversity. However, as he has provided a largely original theoretical synthesis he cannot be seen as a mere conduit for European theory. Yet, it is unclear how influential he has been within Canada. His recent programmatic statements (Trigger 1990; Trigger 1991) are relevant for Canadian archaeologists; it will be interesting to see whether they will be influential.

POTENTIAL EUROPEAN INFLUENCE IN TWO REGIONAL ARCHAEOLOGICAL TRADITIONS

European influence might have been expected to be particularly strong because of their colonial histories in two areas of Canada: Quebec and British Columbia.[12]

Quebec

The development of archaeology in Quebec differs in important ways from that of the rest of Canada, and it reflects closely the distinctive provincial history. However, contrary to what might be assumed, there is not an appreciable influence from French archaeological theory. It must be remembered that Quebec has not been a French colony since the mid-eighteenth century. The most immediate, poignant and enduring colonial history is that of the period of British (and, by extension, Anglo-Canadian) domination. While there is a shared language with France there is little shared culture, nor has there been any significant interest on the part of French scholars in the prehistory of Quebec. As the leading Quebec archaeologist Clermont (1987:849) has summarized the relationship:

> La France elle-même est géographiquement trop loin et son passé est culturellement trop différent du nôtre pour que les Québécois puissent raisonnablement y voir autre chose qu'un lieu de rencontres occasionelles et de débats généraux.

It is probably fair to characterize most Quebecois archaeology as an anthropological 'Americanist' archaeology, conducted mainly in the French language. But, while reading lists in Quebec are dominated by English-language titles and anglophone authors, Quebec universities produce archaeologists who are the best read in French literature of any in the New World (Clermont 1987: 849–51).

Professional archaeology in Quebec started later than in the rest of Canada. Smith (1974) suggests that a combination of Quebec nationalism and Roman Catholicism helped create the image that Natives within Quebec were barbarous 'Sauvages'. This image was created and maintained through the writings of the leading nineteenth century historians, Francois-Xavier Garneau and Abbé Ferland, whose textbooks were widely used in the schools and were influential well into the twentieth century. Attitudes towards Natives had swung from 'Noble Savage' to 'Savage Savage' over the first few hundred years of Quebec history. This history was first written as a coherent narrative when union of the British colonies of Upper Canada (Ontario) and Lower Canada (Quebec) was effected in 1841. Garneau's nationalism increased with this union, which raised fears for the survival of a French Catholic culture in Quebec (Smith 1974:28). The resultant popular surge in anti-Native nationalist sentiment probably had mixed roots in loathing and empathy, as foreshadowed by the early eighteenth-century writer Chateaubriand, who stated that the French in North America were

> a doomed race, destined to dwindle away like the aborigines with whom they have intermingled and sympathized.
>
> (Chateaubriand, in Smith 1974:28)

Furthermore, there was considerable opposition by the church to the study of natural history, and religious and nationalistic sentiment retarded the romantic view of Native Indians which elsewhere encouraged amateur archaeology (Trigger 1986:193). Hence, Quebec experienced little of the antiquarian tradition which may act as an important stepping-stone to a professionalized archaeology, or at least to an archaeological subculture within the larger society. Most early archaeological work in Quebec was carried out by anglophone scholars such as Dawson, Lighthall and Wintemberg (Noble 1973). McGill University, a bastion of Anglo-Quebec culture, was the provincial centre for this inquiry. With few exceptions, such as the work of de Saint-Maurice (Martijn 1978:15), there was little involvement by francophone Quebeckers in the province's prehistory. Struggling to maintain their own culture in the face of Anglo-Canadian colonialism, Franco-Quebeckers saw Native cultures as part of the wild terrain to

be tamed and civilized (Girouard 1977:159; Martijn 1978:11). The native people were used as a device, symbolizing what could happen if rural Quebeckers abandoned rural life and Catholicism (Smith 1974:101). Once an indigenous Quebecois archaeology did become established in the 1960s it is said to have played a role in combatting this stereotype of 'le Sauvage' (Martijn 1978:18), and continues as a leading agency in the search for common ground with the Native communities (e.g., Denton & Duguay 1993).

To the extent that Quebec society feels politically threatened, it should engage in an increasingly nationalistic archaeology which focuses on recent Quebec historical sites (Trigger 1984:361).[13] While there is little overt evidence for this, there may be greater interest in urban archaeology within Quebec than elsewhere in Canada. This impression is bolstered by francophone dominance of the urban sessions at the 1993 Canadian Archaeological Association (CAA/ACA) meetings. In Quebec, and perhaps even more so in western Canada, the history and historical archaeology of the fur trade is also overtly nationalistic (Burley 1994:83). This is perhaps best seen in the context of the national myth that Canada's colonial history (including westward expansion) was generally peaceful, in contrast to the 'violent imperialism' south of the border. Nevertheless, specific cases in which archaeological evidence has been used in a political arena to support Quebecois nationalism are rare or non-existent. It could be argued that prehistory is unlikely to be co-opted by nationalists because it supports alternate sovereignties.[14] Indeed, this may explain why the Quebecois did not form a closer cultural alliance with the First Nations to oppose the British colonialism they were both subject to. Noting the difficulties colonized minorities pose for nationalists, Plumet (1984:43) states that:

> Plusieurs figures militantes des débuts de l'archéologie amateur québécoise étaient des indépendantistes convaincus. Certains sont maintenant pris dans l'étau des contradictions du souverainisme québécois confronté a l'ethnicisme amérindien.

Chapdelaine (1978:5) has invoked the potent Quebec nationalist slogan *'Je me souviens'* when arguing for the *inclusion* of Native prehistory within the collective memory and common heritage.[15] Plumet (1986:28–9) has argued this more directly, noting that in France everyone from Neanderthals through the Greeks to the Franks are incorporated within a unified national heritage. He had earlier (Plumet 1979:201; Plumet 1984:44) constructed similar lists of overlapping past identities associated with the archaeological heritage of south-eastern Quebec.

The development of archaeology in Quebec seems to have followed two general themes of provincial history: the leading role of the Catholic Church in political and cultural affairs until the 1960s, and an early intellectual domination by anglophone Quebeckers, Canadians, and Americans. Prehistory has not been directly invoked for nationalistic purposes. In essence, the archaeology of a

multi-ethnic state, whether Canada, Quebec or indeed Russia or Brazil, is difficult to use for unifying nationalistic purposes unless that nationalism is bound to an ethos of multiculturalism. Selected slices of the archaeological record can, however, be used by segments of the population, elite or otherwise, to either bolster or suppress nationalistic sentiment. The past should not be used to justify the present, and archaeologists will need to abandon their 'feigned neutrality' and accept responsibility for the uses to which others put their work (Plumet 1984:45). The most obvious examples of this are judicial land-claim disputes (see below). Archaeologists can play either an aware or an unwitting role in nationalistic developments within Canada; the former seems preferable.

Coastal British Columbia

The archaeology of the Northwest Coast, including coastal British Columbia, has been described as being, until recently, 'fundamentally atheoretical' (Maschner & Fagan 1991:921). In their usage, 'atheoretical' is apparently meant as anything outside the generalizing, processualist paradigm which, in any case, has seldom if ever been rigorously applied in the region. There has been a little interest in theory, notably Nash's (1983) attempt to categorize prior research by borrowing from David Clarke's (1972) *Models in Archaeology* before proposing a model of 'dialectical evolution'. This is, perhaps, the most explicitly theoretical contribution to Coastal British Columbian prehistory. Far more typical are studies which focus on subsistence and ecology based on highly detailed portraits of one or a few key sites, using uncritical applications of the direct historic approach (Maschner & Fagan 1991; Fladmark 1982:101; Carlson 1983).[16]

The nature of this attention to detail places Northwest Coast archaeology firmly into the holistic, historical particularism of Boasian anthropology.[17] Boas, whose ethnography dominates the region, was a German by birth and by training a geographer and physicist (Stocking 1966). Stocking (1966:871) and Gruber (1986:176) have noted that Boas's concept of culture was rooted in a European tradition. Gruber (1986:176) characterizes this as

> a product of an earlier, persistent, and essentially German theme born of German Romanticism in which a human group, the Volk, possessed a historical unity and particularity expressed in the total range of everyday behaviour.
>
> (Gruber 1986:176)

Reconstructing prehistoric cultures in this European-derived paradigm of enquiry necessitates a holistic approach which can only be realized archaeologically through massive and direct use of ethnographic analogy. Ethnohistorical and ethnoarchaeological work, largely performed by archaeologists,[18] has built on the strength of the minutely detailed Boasian corpus with the aim of ever-improved application of the direct-historical approach. Such an approach has probably

seemed feasible because most excavations have been in shell middens which, with their characteristically good preservation of faunal remains and perishable artefacts, offer alluring detail on this total range of everyday behaviour.[19] Prehistory on the Northwest Coast thus frequently consists of a simple stacking of ecological snapshots. Only rarely (e.g., Ames 1991; Marshall 1993) are the processes of continuity and change given equal prominence.

But for Native people, the anthropologist-archaeologist's search for internally consistent, holistic, cultural behaviour has required a 'remembered past transformed into an ethnographic present' (Gruber 1986:177). The challenge on the Northwest Coast is to escape simplistic analogies from the constructed ethnographic present without falling into either timeless, ecological generalizations lacking historical context, or seamless forward marches of progress.

Inevitably, an historical approach will include episodes such as the expansion of one First Nation at the expense of another, such as (proto) Wakashan conquest of (proto)Salishan territory tentatively suggested by Mitchell (1988).[20] This is the drawback of a humanistic, historical archaeology in Trigger's (1990) sense: skeletons can come rattling out of the closet proclaiming that European domination may be seen as differing in degree, rather than kind, from what went before. Grahame Clark forecast this in his address to the Canadian archaeological community entitled 'New Perspectives in Canadian Archaeology: a Summary':

> The process by which one culture gets incorporated or absorbed in another resembles that by which one phase of the same culture is replaced by another. Regrets are vain. One cannot stay the hand of history. One can only mitigate the brutality of the process.
>
> (Clark 1977:242–3)

It is now clear that 'the hand of history' will eventually hold an archaeologist's trowel. Clark's fatalistic, disengaged attitude is not appropriate for Canadian prehistoric archaeology (and see Evans 1995).

LINGUISTIC RELATIONS IN THE CANADIAN ARCHAEOLOGICAL COMMUNITY

A recurring theme in Canadian culture is the concept of the 'two solitudes', French and English, a dichotomy stemming directly from the national mythology of Two Founding Nations, and the concomitant annulment of the First Nations' prior existence.

Relations between French—and English-speaking archaeologists are, apparently, very good, but this statement is necessarily based on informal personal communications as little has been written on the subject. Nevertheless, there is a certain professional distance between the groups in general, reflecting

perhaps the size and diversity of the country as much as the linguistic barrier. This is well illustrated by publications in the *Canadian Journal of Archaeology/ Journal Canadien d'Archéologie,* published by the CAA/ACA, which is officially bilingual. However, a check of the contents of the *CJA/JCA* from its inception in 1977 to the 1991 issue reveals a strong bias in favour of articles printed in English. Of 183 articles published over this fifteen-year span, 175 are in English, overwhelmingly written by anglophone archaeologists. Only eight (4 per cent) are published in French. Similarly, of 138 books reviewed, only six (3 per cent) are in French, and all eleven obituaries are of anglophone archaeologists. The first bilingual contribution was by Wylie (1993).

French-speaking Canadian archaeologists obviously submit their articles elsewhere, mainly to the regional journal *Recherches Amérindiennes au Québec (RAQ)*. Possibly this is born of a desire to promote prehistoric archaeology within Quebec, a step presumably taken with the knowledge that it may handicap wider readership within North America and elsewhere.

A recent article on sedentism in Iroquoian prehistory in the widely circulated, generalist *Journal of Anthropological Archaeology* by the Quebecois archaeologist Chapdelaine (Ph.D., University of Montreal, 1988) illustrates the Canadian publication dichotomy. This article (Chapdelaine 1993) was translated for publication from the original French and is firmly Americanist in flavour. The study area is both international and inter-provincial. Fourteen of the total 137 references are in French, of which ten are from *RAQ* while none is from the *CJA/JCA*. Not more than one or two references in this article are written in English by francophones, reinforcing an impression that Quebecois archaeologists mainly publish in the French language.

FUTURE DIRECTIONS

Canadian archaeology is being performed in an increasingly critical environment. This criticism has come from the First Nations (e.g., Denton & Duguay 1993), from other academic disciplines (e.g., Wickwire 1992), and from within the archaeological community itself (e.g., McGhee 1989, Bielawski 1989). Natives are becoming empowered with decision-making about their heritage. For example, the Aboriginal Heritage Committee of the CAA/ACA, of which one third of the founding members are Natives with archaeological training, is examining ethical guidelines for the profession (Simonsen 1993), modelled on those of the World Archaeological Congress (*World Archaeological Bulletin* 1991:22–3). Their orienting goal is to 'present a series of strong statements which legitimize the discipline of archaeology, both in the eyes of archaeologists and aboriginal people, but in particular with regard to the latter' (Simonsen 1993: 4). Similarly, academic and consulting archaeologists in British Columbia have recently engaged in formal dialogue with the First Nations (Feddema 1993). This follows another recent opportunity for direct feedback which was the invitation to Nuu-chah-nulth (Nootka) elders to speak at the 1992 Northwest

Anthropological Conference in Vancouver, in the context of a session on the archaeology of their traditional territory. Their remarks, which unfortunately went unrecorded, were largely critical, but constructive.

Native people are also being encouraged to take direct responsibility for the management of archaeological and other heritage sites. One avenue is through the recent 'Access to Archaeology' programme of the federal government. According to the director of the programme, successful applicants (who must be affiliated to First Nations organizations) are not merely pawns in cheap labour initiatives dressed up as training (Snow, in Denton & Duguay 1993:93). In a similar initiative, Simon Fraser University operates a programme on the Kamloops Indian reserve (ironically situated in the former residential school), with instruction in a variety of subjects such as ethnobotany, oral history, and linguistics, and which includes an archaeological field school (SCES/SFU 1992). Both these programmes attempt to remove archaeology from its institutional setting. Indeed, archaeology's location within anthropology departments may be one of the greatest obstacles to more active Native participation. The rejection of historical views of the past in these departments 'may help to explain why so few native people want to become archaeologists or are interested in the results of archaeological research' (Trigger 1986:206). These are all laudable efforts to empower the Native communities with control over their own pasts, and should bear fruit in the future.

Grahame Clark remarked (Clark 1977:237) that Canadian archaeology is not just New, but, literally, 'new', in that it 'retains its innocence', and is 'still concerned with archaeology'. This state of affairs is no longer true. Canadian archaeologists now find themselves deeply implicated in the most potent political and ethical current national debate: the judicial settlement of Native land claims and the establishment of aboriginal title and self-government. Most land-claims cases involve establishing native use of an area as far into the past as possible, in order to establish the legal principle of 'occupation since time immemorial'. Archaeological evidence, as well as that of ethnohistorians, historians, anthropologists and Native elders, is presented to establish as complete a picture of connection to the disputed territory as possible. Contract archaeologists hired either by Native organizations or by government are the most overtly connected to this process. Land claims and other land disputes constitute a major source of employment for this group of archaeologists, and there is the potential for archaeologists to be co-opted by one side or the other (Wickwire 1992), although most contract archaeologists eventually work on both sides of the issue. Academic archaeologists may also find their work has unsuspected relevance to a political dispute, often years after its completion. Most of British Columbia, for example, is not covered by treaties and is subject to claims, often overlapping, by the various First Nations. It follows that any archaeological research in the province, no matter how seemingly innocuous or disengaged, will at some point have implications for land claims. Archaeologists must be aware that their research agenda is necessarily tied to the search for

solutions to land disputes (and see Moser 1995). To paraphrase Fisher (1992:xxiii), whether we like it or not, what we say matters, and can profoundly affect the hopes and aspirations of living people. Therefore, both the commercial agenda of contract archaeology and the myth of 'pure research' will have to be modified to fit the new social reality.

In a broader context, archaeological research on land claims implies the rearward projection of existing bounded cultural entities. The linking of 'a people' to 'a land' is the essence of nationalism. In the present world-wide climate of increasing nationalism, Europeans might benefit from a better understanding of the complex definitions of ethnicity and subtle interpretations of sovereignty and self-definition which are debated in Canada. Professional archaeologists in Canada are well situated to take a leading role in building common areas of understanding with indigenous peoples everywhere. Canadian and European archaeologists may, for the first time, be finding common ground as both encounter the problems of contested pasts in multicultural states.[21]

CONCLUSIONS

In the past, Canada has benefited only indirectly from European theoretical developments, while Europeans have paid scant attention to Canada as a discrete entity. Canadians would gain from greater direct contact with the archaeology of other countries apart from the United States. This may be difficult to achieve: as in most cultural affairs the archaeology of the United States projects a penetrating light while casting an accordingly deep shadow. Conversely, Canadian archaeologists and, indeed, members of the Native communities, should be able to contribute to the growing global debate on how to accomodate multiple nations within single states.[22]

Archaeology in Canada, as elsewhere, is undergoing a certain crisis of confidence, traceable mainly to a changing relationship with the native communities. Increasingly, the voices of the First Nations of Canada are being heard, and a dialogue is being established between them and professional archaeologists. In this way Canadian archaeology will come to reflect the aspirations and interests of the native communities. The most desirable path is a more historical approach, eliminating the largely artificial distinctions between history, prehistory and ethnography.

Aside from archaeologists the message from stakeholders in the past is clear, as Wylie (1993:10) has recently and forcefully stated:

> The archaeological record is too important to the framing of a collective historical understanding and to the articulation of cultural identities long under siege, to be treated as the special preserve of small, elite, almost exclusively white, middle class (in our case, Euro-Canadian), and largely male community of investigators whose main claim on the record is

precisely the 'disinterested' (i.e., unbiased/unvested) nature of their interest in it.

The challenge facing Canadian archaeologists is the creation of a new archaeology which is both moral and relevant within the context of a multicultural state. Such new priorities must not merely be taught as method, like straight sidewalls, or transmitted as received wisdoms. Rather, the new dialogue must inform and thereby strengthen every stage of archaeological research and interpretation.

NOTES

1 In this light, it is not surprising that the two leading exponents of Darwinian human ecology, Eric Smith (e.g., 1991) and Bruce Winterhalder (e.g., 1977), have carried out their fieldwork in Canada.

2 Mexican scholarship was similarly excluded from North America in this volume.

3 Trigger has elsewhere (1981:79) noted the distinctiveness of some aspects of Wintemberg's work relative to the United States.

4 Those hired included, with their doctoral institution in parentheses, Jacques Bordaz (Columbia), Charles Borden (California, in Germanic studies), Alan Bryan (Harvard), Roy Carlson (Arizona), Nicholas David (Harvard), Richard Forbis (Columbia), Ruth Gruhn (Radcliffe), Fumiko Ikawa-Smith (Harvard), William Irving (Wisconsin), Jane Kelley (Harvard), Donald Mitchell (Oregon), Richard Pearson (Yale), Bruce Trigger (Yale), and James Tuck (Syracause).

5 These included George MacDonald (Yale), J.V.Wright (Wisconsin), R. Morlan (Wisconsin) and R.S.MacNeish (Chicago).

6 By comparison, most UK departments of archaeology have overwhelmingly or exclusively UK-trained faculties.

7 Such as, among many others, Norman Clermont (Montreal), Brian Hayden (Toronto), Knut Fladmark (Calgary), David Meyer (McMaster) and Gregory Monks (British Columbia).

8 Such as R.G.Matson (California), Michael Blake (Michigan), Louis Allaire (Yale) and Alison Wylie (State University of New York, in philosophy).

9 Including Nicolas Rolland, Aubrey Cannon and M.A.P.Renouf (all Cambridge), and Patrick Plumet (Paris).

10 Important exceptions are francophone universities in Quebec, whose anthropology and sociology faculties were largely trained in Europe.

11 In general, see Wright 1977 and associated comments, especially Plumet 1977 and Trigger 1977.

12 Other areas are not considered in this chapter. In particular, Arctic prehistory—which has seen a great deal of European, mainly Scandinavian, interest—would also make an interesting case study.

13 Plumet (1984:43) suggests that indigenous Inuk archaeology developed for these very reasons.

14 For example, Marois (1975:56) has called for the political divisions between Ontario and Quebec to be disregarded for archaeological purposes.

15 Trigger (1978:166–7) responds to this by suggesting that hundreds of years of injustice prevent any Euro-Canadian, including archaeologists, from appropriating the Native and Inuit patrimony.

16 This makes an interesting comparison with the comments of Mitchell & Donald (1988:342) who claim that the direct historic approach has been used by 'only a few researchers' and deserves more application. Trigger (1991) also insists that a sophisticated direct historical approach is the way of the future for the archaeology of living traditions.

17 However, Pinsky (1992) suggests that, as archaeology was marginal to the Boasian programme, specific instances of influence must be demonstrated. While Wright (1985:424) believes Boasian anthropology '…was, and to a large extent still is, the intellectual template of Canadian archaeologists', this needs to be demonstrated region by region.

18 Wickwire (1992) has blamed archaeologists for not doing more ethnography.

19 Shell middens may have a closer connection to the natural-historic approach. As Hinsley (1989:85) has noted, '[Florida] shell heap archaeology offered an effortless transition from natural history to human prehistory, for the observational skills necessary for analysis, identification, and enumeration of shells and bones of animals, birds, and fish were easily transferred to stone implements or potsherds'.

20 Clearly, this relates to the multiple heritages and the uses of the past in the present, discussed above in the Quebec context.

21 European archaeologists have been occasionally hostile towards Native issues. For example, Bray (1985:449) included in his list of 'Parish Pump' issues the negotiation of the Society for American Archaeology with Native groups and the maintenance of professional ethics. Furthermore, he 'grudges every page and every dollar given over to such parochial matters as…how to placate the Indian lobby' (Bray 1985:448).

22 For example, Fleras & Elliot (1992:220–31) believe that the Canadian political structure is particularly suitable for a flexible interpretation of sovereignty within the state.

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CHAPTER NINE
THE SOCIO-POLITICS OF THE DEVELOPMENT OF ARCHAEOLOGY IN HlSPANIC SOUTH AMERICA

GUSTAVO POLITIS

INTRODUCTION

The formation of archaeology as a scientific discipline in South America has been quite different in each country.[1] Although it is possible to point to some similarities, the continent-wide development of the discipline has been heterogeneous. When we talk about quintessential Latin America, most people, including scholars, imagine countries such as Peru or Bolivia, with large populations of indigenous peoples, or perhaps Brazil (see Funari 1995) and Colombia, where the population comprises a mixture of indigenous peoples and those of African or European ancestry. In contrast to the above examples, Argentina has a large population of European descent and one of the smallest indigenous populations in South America. Nevertheless, as an example of the development of archaeology in the region, Argentina is no more or less 'typical' than any of the other countries. In Argentina political changes have been extreme, and their impact on archaeology is perhaps more clear-cut than in the other countries.

The Argentinan case study, then, firmly rooted in its Latin-American context, allows us to reflect on how archaeological knowledge is constructed, and to what extent the national-political context and its place in the international arena affects both practical and theoretical archaeology in a given country. I also refer, but in less detail, to some of the political contexts of other countries, especially Peru and Colombia, because they help to clarify the socio-politics of archaeology in the region from a wider perspective.

Spanish South America comprises many countries, forming the fourth largest continent in the world with, in 1985, about 132 million inhabitants. Although the main language is Spanish, indigenous languages are quite widespread in some countries (i.e., Peru, Bolivia, Colombia, etc.). All of the countries in Spanish South America share a strong Catholic tradition.

These states were former Spanish colonies which became independent in the early decades of the nineteenth century, after several years of war against Spain. Two men, San Martin from Argentina and Bolivar from Venezuela, led the revolutionary forces, which included soldiers from several South American

countries fighting in close union until they liberated the continent in 1828. Subsequent to this liberation there was a period of violent civil wars in the mid-nineteenth century, as peoples of different regions sought to consolidate their states and political organizations, on the basis of new constitutions. The second half of the nineteenth century was characterized by border disputes between South American countries and also by new colonization of indigenous peoples' territory. During this period there was no room for systematic archaeological research, and the few recorded observations of archaeological remains (almost exclusively monumental) were made by foreign visitors (e.g., Humboldt 1814, Rivero & Tschudi 1851, Bollaert 1860).

THE RISE OF NATIONAL ARCHAEOLOGIES H AEOLOGIES

Scientific archaeology emerged in South America in the last decades of the nineteenth century. This was also the time when the new ruling elites thought they could best develop their new nations by drawing as fully as possible on foreign enterprise, foreign investment and foreign culture. They increasingly looked to the United Kingdom, France and the United States for models (Whitaker & Jordan 1966).

In Argentina archaeology was born as a science at the end of the last century during the time of the so-called 'generation of the eighties', when the ruling elites were preoccupied with an idea of 'progress', promoting European cultural values rather than indigenous and *criollo* (creole) values. This idea of 'progress', which was related to early concepts of evolution, encouraged the development of science not only as a tool to improve knowledge about the country, but also as an intellectual exercise. It was also an attempt to mimic the behaviour of the more developed European countries, especially France and the United Kingdom, which were used as models. During this period, the national schools of natural sciences came into being and were strongly promoted, the first anthropological research began and some of the first archaeological papers in the country were published. Two of them were highly significant: one, written by Liberani and Hernández (1877), was the first 'catalogue' of the 'Indian antiquities' of the Northwest of Argentina, the other was by Florentino Ameghino (1880), who gained an international reputation by proposing that the first human beings appeared in the Argentine pampas during the Tertiary period. In keeping with the scientific conventions of the time, the first edition of Ameghino's book was also published simultaneously in France and, of course, in French. Ameghino was deeply influenced by Darwinian evolutionism and was its first advocate in the southern hemisphere (Politis 1988). Evolutionism was in tune with the political perception of progress. Late eighteenth-century governors believed 'progress' meant changing the face of the country, through progression from indigenous and traditional ways of exploiting resources to more developed intensive processes that would enable Argentina to enter world markets as a

major producer of raw materials. For them, progress meant populating the country with European immigrants (they naturally considered territory populated by indigenous peoples to be empty and called it 'the Desert'). The keys to the progress of the country were Europeans, private land ownership and railways to take crops and meat from the interior to ports, for export to Europe. The early Euro-Argentines (those who were of European origin but lived in Argentina) and local anthropologists had no difficulty in persuading governors to help them make their dreams reality. Large museums and important scientific expeditions were funded and significant resources were allocated to research. The evolutionary ideas of Ameghino were developed at the very moment when the state of Argentina was being consolidated, when notions of progress, evolution and struggle for life fired the social imagination of the ruling elite.

Between 1879 and 1881, the national government sent several military expeditions to the huge territories of the pampas and Patagonia in the so-called 'Conquest of the Desert', areas which, until then, had been the territory of the Mapuche and Tehuelche indigenous peoples. Some scientists (e.g., Zeballos 1960; Zeballos 1978) accompanied these expeditions in order to study the fauna, the flora, the landscape, and to collect the heads of dead indigenous people for bio-anthropological purposes (see the discussions in the *World Archaeological Bulletin* 1992). Following military conquests, British railway companies brought 'progress' by developing a rail network, centred in Buenos Aires. It is important to note that the construction of the Museo de Ciencias Naturales de La Plata (one of the largest museums in South America) began just as the military campaign against the indigenous people ended. Obviously a place to store and exhibit the recent achievements of the government was required. The architectural styles and exhibition halls reflected the European tradition of museum construction, and the Smithsonian Institution in the United States was also taken as a model, since that museum had also been conceived as a place in which human diversity could be studied and investigated.

At the beginning of this century, local archaeological research flourished in Argentina, thanks to government support. Local archaeologists attempted to mimic the intellectual achievement of some European countries: Ambrosetti (1897; 1902; 1906) developed pioneering stratigraphic research in the Northwest, Torres (1911) excavated mounds in the Paraná delta, and Outes (1908; 1909; 1916) and Debenedetti (1912) were active investigating the archaeology of the pampas, Patagonia and Northwest regions. Some foreigners also undertook research in the Northwest, among them the Swedish scholar Boman (1908), who lived in the country for several years, and Rosen (1904; 1924), a Swedish count.

In other Latin American countries, archaeological investigation was beginning in a very different way. In Mexico and Peru, early archaeological research was typically carried out by visiting foreign scientists attracted by monumental archaeology. In the Andes, especially in Bolivia and Peru, M.Uhle, a German archaeologist, was the outstanding figure of the time. Uhle (see Willey & Sabloff

1980) introduced the concept of 'horizon style', although he did not define or explain it, and he devised the first chronologies in the Andes based on Inca and Tiahuanaco remains. In Bolivia, Nordenskiöld (1913), another Swedish aristocrat, carried out excavations in several lowland mounds and, on the basis of stratigraphy, distinguished two chronologically defined cultures.

It was a Chilean, José Toribio Medina (1882), who laid the foundations of archaeology in that country, following on the work of Bollaert. Meanwhile, Ameghino visited Uruguay in 1875–1876 and undertook some research there (Ameghino 1880). Shortly after that visit, Figueira (1892), a pioneer who carried out excavations and engaged in research in many parts of the country, wrote the first comprehehsive work on the prehistory of Uruguay, contradicting many of Ameghino's conclusions (Toscano n.d.).

In Colombia, Zerda, an historian, published a study about the Chibchas (Zerda 1882) and, shortly afterwards, Restrepo (1895) produced a monograph on the same group. It was not until 1913, however, that systematic archaeological research was undertaken in Colombia, when Preuss—from the Museum für Völkerkunde (Berlin)—started excavations at San Agustín (Reichel-Dolmatoff 1965). Although his fieldwork and research had been planned to last for only a few months, Preuss was forced to remain in Colombia for six years, due to difficulties in returning to Germany as a consequence of World War I (Uribe 1987).

In most parts of Latin America, therefore, cultural evolution was the dominant interpretative paradigm, and the most obvious instances of cultural contacts and diffusion were ignored (Willey & Sabloff 1980). Only Argentina initially developed any kind of local archaeological tradition (based in part on Argentinians of Italian descent). Elsewhere, knowledge of the South American pre-Hispanic past was essentially controlled by foreigners (mainly European) who usually published only in their own languages. They determined the scientific priorities and the problems to be discussed. Andean archaeology was led by well-educated German scientists and Swedish aristocrats.

THE SPREAD OF DIFFUSIONISM

The decline of evolutionism everywhere at the end of the nineteenth century (Trigger 1989) brought to Latin America new ideas which developed along different paths in each country. Archaeology acquired historical significance thanks to the quest for chronologies with which to systematize the pre-Hispanic sequences. At the beginning of this century, archaeologists once again sought to reinforce the links between their discipline and national histories, and scholars paid more attention to the geographical distribution of types and clusters of artefacts, trying to associate them with historical groups (Trigger 1989). The main supporters of the study of the distribution and the chronology of archaeological remains were the Swede G.Montelius, the German G.Kossinna and the Australian V.G.Childe. Although some stratigraphic excavation had

already been carried out (e.g., by Manuel Gamio in Mexico and Max Uhle in the San Francisco Bay) this was the time of the 'stratigraphic revolution', when chronologies were derived from field data (Willey & Sabloff 1980). In Latin America, culture-historical syntheses of regions and areas became the main objective, involving a direct historical approach. Classification and typology were the core archaeological methods. In this context, the idea of diffusion emerged as a key concept.

During the early twentieth century the United States expanded its political influence and economic interests in South America. The *criollo* landowner elites were becoming increasingly weak in face of the representatives of North American companies. In some countries this process led to the rise of an incipient urban middle class. At the turn of the century it is clear that South America passed from an era of European intervention to one of North American tutelage. A clear-cut example of this is the 1902 conflict in Venezuela, when three European countries, the United Kingdom, Germany and Italy, blocked Venezuelan ports claiming a debt payment (Halperin Donghi 1972). With the growth of North American economic and political interest in the region, the old European powers began a cautious withdrawal; only Great Britain resisted longer, into the early twentieth century, and Germany, whose influence was important (especially around the Caribbean) until the beginning of World War I. The United States did not go as far as military intervention in South America, as it had done in Central America and the Caribbean; its means of domination were more subtle, and concentrated on political and economic pressure.

At the end of the nineteenth and beginning of the twentieth century, the structure of Argentine society began to change, as a result of massive immigration from Europe (mainly from Spain and Italy). The indigenous peoples no longer constituted a problem, since the national state had effectively taken control of their territory (Podgorny & Politis 1989). At the same time, conflict was emerging due to the arrival of working-class immigrants bringing with them anarchist and socialist ideas. Interest in the origin of humanity and its evolution declined slowly after Ameghino's death, and was replaced by an increased awareness and promotion of indigenous and Hispanic-American *(criollo)* traditions. This shift provided the ruling elite with a tool to control immigration: they needed to reinforce the Hispanic and *criollo* tradition in order to distinguish themselves from the immigrants and, in doing so, to claim 'acquired rights'.

In this context, archaeological research was oriented towards the use of historical sources. In spite of the considerable amount of systematic work carried out by Argentines, there was still little chronological systematization in comparison with other countries in the region. As a consequence, historical sources were overused and a tendency emerged to force chronology towards the most recent pre- Columbian periods. A remarkable exception was the pioneering work carried out in the Magallanes (Magellan) Strait by the North American Junius Bird (1938, 1946), who suggested there had been long-term human occupation in the area dating back to late Pleistocene times.

In the meantime, ideas from the Vienna School, based on the so-called *Kulturhistorische Methode,* began to spread to Argentina through lectures, papers and books written in Spanish by José Imbelloni. Imbelloni, born in Italy, visited Argentina for a short period and returned to Europe as a volunteer soldier during World War I. After the war he studied in Italy, obtaining a doctorate in natural sciences, with which he returned to Argentina in 1920 (Arenas & Baffi 1991–2). The same year he was appointed to a professorship at the University of Buenos Aires, from where he became influential among subsequent generations of Argentine anthropologists. Although Imbelloni was in the main a physical anthropologist (Imbelloni 1924–5; 1933), he also discussed anthropological theoretical issues in a wide-ranging book (Imbelloni 1936). It was in this publication that he developed the term *Culturología* ('culturology')—synonymous with the German *Kulturmorphologie* and the French *ethnologie culturelle*—that was based on the main principles of the *Kulturhistorische Methode* and that allowed him to articulate his theories about culture, society and diffusion.[2] His main objective was to provide 'a general idea about the relationship between man and civilization' (Imbelloni 1936:22). Cultural evolutionism was strongly criticized, especially the work of Sir Edward B.Tylor (1871), while Austro-German diffusionism was supported.

Imbelloni proposed three main characteristics or guidelines for the definition of 'cultures': (a) the outstanding originality of their component elements, (b) the constant association of their elements, and (c) the cultural traits used to define a 'culture' had to belong to all sectors of human activity. In this sense, each 'culture' was considered as a 'type of civilization' and had two connotations: a territory and a patrimonial content.

Through his definition of eleven main 'cultural circles', Imbelloni introduced into South America the ideas of Oswald Menghin, the Austrian prehistorian who proposed the concept of 'primordial culture' *(Urkultur)*.

This introduction provided fertile soil for the subsequent development of post-war archaeology. The triumphant arrival of the Vienna School occurred long after the crisis of evolutionism, at a time when there was no dominant paradigm in Argentina's archaeology; this school of research became popular, and even liberal-oriented archaeologists did not react against it (González 1985).

In the 1930s Argentina underwent a series of military coups, a characteristic way of siezing power which remained intrinsic to Argentine politics until as late as the last decade. Reactionary conservative governments of landowners, backed by army officers and opposed to working-class communities, continued to hold sway until 1946 (Halperin Donghi 1972). During this period Metraux, a well-known French researcher and former student of Paul Rivet, arrived in Argentina bringing with him not only his high prestige but also reinforcement of the historicist paradigm. Canals Frau, of Spanish origin but trained in anthropology and ethnology in Germany, also reached the country and concentrated on the analysis of historical texts, while also translating one of Graebner's main papers (Lafón 1958–9).

In terms of the theoretical frameworks adopted, Uruguay was quite similar to Argentina at that time. In 1926 the Sociedad Amigos de la Arqueología was founded, bringing together prestigious intellectuals and politicians of the day. The Society published a journal in which the articles reflected the historicist orientation of the discipline. Within the humanities, archaeological findings were interpreted with reference to historical accounts in order to associate them with historically attested aboriginal groups (Cabrera Perez 1988). Exceptionally among scholars of this period, local palaeontologist Francisco Berro, a strong supporter of Ameghino's model of coexistence between humans and megafauna, continued to claim a great antiquity for the human occupation of the country (referred to in Toscano n.d.).

The rest of South America increasingly adopted a culture-historical orientation. Uhle left Peru in 1911 and began working in Chile and Ecuador, adopting a diffusionist approach and ending up obsessed with his search for evidence of Maya invasions into Ecuador and Peru (Collier 1982). While in Chile, and still influenced by the Vienna School (Uhle 1918; Orellana 1974–5), Uhle made three major contributions to its archaeology: (a) the development of the first chronological chart, (b) the description of the Atacameña culture and its contribution to some stylistic traits of Tiahuanaco, and (c) the identification of a Tiahuanaco period in northern Chile (Orellana 1974–5). He also applied some seriation in order to organize the chronology of the materials and acknowledged that 'types could change through time' (Willey & Sabloff 1980).

In his last Ecuadorian writings, Uhle postulated that the Middle American Maya were the ancestors of the American Higher cultures (Uhle 1922a; Uhle 1922b; cf. Willey & Sabloff 1980). It had been Jijón y Caamaño, a bibliophile and ethnohistorian who, stimulated by Rivet's work between 1901 and 1906, and himself from an aristocratic family from Quito, personally sponsored Uhle to carry out fieldwork in 1919 in Ecuador (Collier 1982). As a result, Uhle 'had a diversionary or refractive effect on Jijón y Caamaño's archaeological focus' (Collier 1982:8). Some years after Uhle returned to Germany, Jijón y Caamaño (1951) produced his synthesis of Ecuadorian chronology and pre-Hispanic cultures which provided the foundation for a diffusionist approach in Ecuadorian archaeology.

Meanwhile in Peru, Julio Tello had rejected Uhle's cultural sequence and he, together with Rafael Larco Hoyle, was working in the Central Andes, which had became a kind of laboratory where North American culture-historical archaeologists had begun to experiment with their methods and theories. Larco Hoyle (1938–9, 1946) supported the hypothesis of coastal origins for Peruvian civilization (McGuire 1992) basing his interpretations on Gordon Childe's concept of the Neolithic Revolution (Patterson 1989). Tello, on the contrary, argued for an Andean origin.

Tello, from an indigenous family in the central highlands (Dagget 1992), became the most influential of Peruvian archaeologists, exercising a 'kind of monopolistic control over the archaeological research of his compatriot'

(Schaedel & Shimada 1982:360). Already in 1909 Tello had received a Peruvian government grant which allowed him to study for two years in the United States (for an MA in anthropology from Harvard University). Later he was afforded the opportunity to study in important European museums in England, France and Germany (Dagget 1992). He became an early supporter of the *indigenismo* movement developed in Peru and Mexico during the 1920s. This movement was a major manifestation of nationalism in both countries, glorifying the Aztec past in Mexico and the Inca past in Peru, in order to legitimize the unique Indian identities of both countries (McGuire 1992). Strong affiliation with the government of Augusto Leguía (1919–30) helped Tello not only to carry out his research but also to spread his ideas among his Peruvian contemporaries. Leguía was elected President in 1919, but later created a sort of civilian dictatorship until 1930 when he was removed from power by a military revolution. During his government, the *indigenismo* movement became part of a broad programme to develop the country, called 'Patria Nueva'. Leguía's transformations may not have been very profound, but they led to an avalanche of North American investment, accelerated economic expansion and a dramatic increase in public support. The government faced opposition from certain sectors of the oligarchy of Lima, as well as from an eclectic group including university students and *mestizos* which later developed into the Peruvian APRA political party (Halperin Donghi 1972). When Leguía was removed from the presidency in 1930, so Tello lost his post. Later, however, in 1937, Nelson Rockefeller assisted him in the foundation of the Institute of Andean Research (Patterson 1989). When Tello died in 1947, his body was guarded in the Museo Nacional de Antropología y Arqueología of Lima and was conducted to the cemetery with the honours normally accorded to a minister of state. Since then, libraries, streets, plazas and schools have been named after him. Tello was an exceptional case in South America, the unique archaeologist who became a scientist/politician indigenous hero. He was glorified and seen as

> a man of the people, a representative of the indigenous population, and a messenger from the Amautas, the descendants of the ancient Inca empire.
>
> (Mejía Xesspe, in Tello 1967:3)

During this time, a period defined by Willey & Sabloff (1980) as 'the Classificatory-Historical Period', the main objective of North American archaeologists was the culture-historical synthesis of the regions of America. To achieve this goal, several technical and methodological devices were created or adopted, including stratigraphic excavation, seriation of archaeological materials, typology and pottery classification. It is interesting to note that in the development of this 'stratigraphic revolution' (Willey & Sabloff 1980) a Mexican archaeologist, Manuel Gamio, played a major role by excavating in the Valley of Mexico, providing a sequence with which to demonstrate and understand pre-Columbian cultural development. Although stylistic seriation was

pioneered by Uhle in Peru, it is Kroeber and his students who should be credited for making these methods comprehensible to a broader public (Willey & Sabloff 1980). Stylistic seriation, based on the associations of vessels in tombs, was used to derive chronological information from both excavated and looted material, and became a procedure later used in several Andean countries (e.g., Argentina and Chile). Kroeber (1927, 1944) developed a conceptual framework for Uhle's synthesis, expanding and refining it. The work of Kroeber and his associates was a major contribution to knowledge about the pre-Columbian Andes, and laid the foundations for a growing North American influence in Andean archaeology.

After a period of violent civil war, known as the 'War of A Thousand Days' (1899–1902), North Americans started to undertake research in Colombia. There the Conservative Party remained in power until 1930 while retaining several aspects of the nation's archaic structure (Halperin Donghi 1972). During 1922 and 1923, Alden Mason, from the Field Museum of Natural History in the United States, carried out major excavations in the Sierra Nevada de Santa Marta, and published an extensive report in the early 1930s which is still considered a milestone in the archaeology of the area (Reichel-Dolmatoff 1965). Several European researchers (from Italy and Belgium) also worked in Colombia, while the Spaniard J.Pérez de Barrada led the first archaeological expedition to San Agustín, with financial support from the Ministry of National Education (Reichel-Dolmatoff 1965).

During the 1930s the social sciences received a great boost as a result of the Liberal Party coming to power. Presidents E.Olaya Herrera, A.López Pumarejo and Eduardo Santos supported this initiative and energetically promoted education at all levels, developing at the same time a progressive social policy (Chaves Chamorro 1986). At the end of the decade the semi-official Banco de la República inaugurated the 'Museum of Gold' with the aim of reducing the age-old illegal export, and melting down, of thousands of gold artefacts discovered by grave-robbers (Gnecco n.d.). During World War II, in 1941, and during a short-lived period of liberalism, Colombian President Santos offered Paul Rivet, whom the Colombian government had helped to escape from France during the German invasion, premises from which he could pursue his research (Gnecco n.d.). A few years earlier Colombia had taken in representatives of the Spanish intelligentsia who had been exiled from Spain after the Civil War, as well as some German scientists who had been persecuted by the Nazis.

The conjunction of these three groups paved the way for the development of Colombian anthropology. With Rivet, the sociology of Durkheim and Mauss made its way to Colombia, as well as French functionalism (Chaves Chamorro 1986). French influence was increased by the arrival in Colombia in the early 1930s of the French anthropologist Henri Lehmann who was very active and influential during the 'liberal period'. A major local figure was G.Hernández de Alba, who had studied in Paris where Rivet and Mauss had been his professors. In 1935 he set up a North American 'Anthropological Mission' to carry out field research, while in 1937 he supported the creation of the Servicio Arqueológico

Nacional and in 1938 he was the driving force behind the creation of the Museo Arqueológico Nacional. In his early days, his research consisted of a somewhat socially 'neutral' archaeology but, as a result of the dramatic social situation in which the Colombian indigenous people found themselves, de Alba later embraced the *indigenismo* movement, following in the paths of Gamio in Mexico and Tello in Peru (Chaves Chamorro 1986). When Lehmann left his professorship at the Instituto Etnológico Nacional to move to the Universidad del Cauca in Popayán, de Alba moved with him.

Led by Rivet, the first generation of local archaeologists were trained at the Instituto Etnológico Nacional, modelled on the Institute of Ethnology of the University of Paris, which had been created by Rivet and Mauss in 1926. It was divided into four main sections: archaeology, ethnography, ethnology and linguistics. Its first generation of students included the first female South American anthropologists, a direct consequence of the decisive support for women's rights from the government of Santos who, in 1938, encouraged and made it legal for women to attend university. One of these female students, A.Dussan, married G.Reichel-Dolmatoff, an Austrian anthropologist who had arrived in Colombia at the end of the 1930s. Together they were to become the most influential figures in the fields of archaeology and social anthropology in the decades that followed.

Until 1927, Chile enjoyed parliamentary coalition governments led by alliances of the main political forces. Between the wars, the country welcomed two European archaeologists who lived in the country and became Chilean nationals: Richard Latcham (1928) from Britain, who became the Director of the Museo Nacional de Historia Natural in Santiago, and Austrian Grete Mostny (1954, 1971) who arrived in Chile from Belgium (where she had gained her doctorate in prehistory) and carried out much fieldwork in many parts of the country (Durán 1977). However, there was also North American archaeological interest in Chile through the research of J.Bird (1938), who developed sophisticated methods of excavation in southern Patagonia. His excavations at the sites of Fell's Cave and Palli Aike became landmarks in studies of the peopling of America. During World War II, Bird returned to Chile to investigate the northern coast where he established, through excellent stratigraphic control, the cultural development of the littoral fisher-gatherer societies (Bird 1943) and established new criteria which gave an important impetus to the archaeology of the country (Rivera 1983).

AFTER WORLD WAR II

The period which followed World War II saw the consolidation of North America's hegemony in the countries of South America and the region came under the political and economic domination of the United States. This domination was also accompanied by an increasing cultural influence in South American countries. This period, called the 'crisis of the neo-colonial order' by

Halperin Donghi (1972) represented a new step toward the dissolution of links between western Europe and South America.

Julian Steward's (1946–50) *The Handbook of South American Indians* constituted the first large-scale attempt to interpret the archaeology of South America (Roosevelt 1991). The organization of this series reflected the assumption that centres where innovations originated were those which attained the highest level of complexity. Numerous archaeologists, most of them North Americans, were appointed to contribute to this project of systematization.

Juan Perón took office in 1946 after democratic and open Argentinian elections, with the support of the working class, the church and the military. The early years of his government benefited from the prosperity that the war brought to raw material producers such as Argentina. This period was characterized by a high degree of state control of the economy. In the early 1950s Perón began to apply a neo-conservative formula to his rule; nationalism, state control and populism were characteristic of the latter part of his period in office, a time when he lost church and military support (Halperin Donghi 1972).

Under the Perón government (1945–55) universities were no longer autonomous (a right lost in 1930 at the time of the first military coup), and were under direct control of the government. During this period a significant percentage of liberal academics were expelled from the universities and the right wing of the Peronista government increased its control. As a result, Imbelloni became an influential person in the Academy and close to the Peronista government. It was also during the first term of the Perón government that eastern European scholars arrived in Argentina, under official protection, to take up influential positions: Oswald Menghin (Austria) started research in the Museo Etnográfico de Buenos Aires (later to become Professor in the two most important Universities of Buenos Aires and La Plata); Branimiro Males became Director of the Instituto de Arqueología de Tucumán; and Miguel de Ferdinandy (a Hungarian resident in Portugal) became Director of the Instituto de Arqueología y Etnología de la Universidad de Cuyo in Mendoza (see Schobinger 1971, González 1991–2). Of these, Menghin was undoubtedly the most influential. He taught and carried out research, enjoyed academic power and had a prestigious reputation in the field of European prehistory; it was he who extended Kossinna's ideas (the so-called settlement-archaeology method) into prehistoric archaeology (Härke 1991; Härke 1995). His ideas had already been brought to Argentina by Imbelloni, and his political past as an active member of the Austrian Nazi Party (Arnold 1990) was quickly hidden.

While Austro-German diffusionism declined in the rest of the world, in Argentina many students were taught how the *Kulturkreis* travelled from one continent to another, carried on 'population waves'. North American cultural history had very little impact on the theoretical structure of the Argentinan archaeology of the late 1940s and early 1950s. Aligned with a variety of historical approaches to late pre-Hispanic periods, such diffusionism was both monolithic *and* satisfied government expectations. Regional traditions were

reinforced during Perón's rule and local museums were also founded in order to demonstrate to the public those ancient elements which contributed to the formation of the 'national identity'.

By the time that the Peronista government was ousted by a military coup in 1955, a group of North American archaeologists had published a compendium of Argentine archaeology, based almost exclusively on the published literature (Bennett, Bleiler & Sommer 1948). By compartmentalizing ceramics into styles, and artefacts into complexes, they were able to propose a spatio-chronological framework of cultures and periods. As stratigraphic research in northwest Argentina intensified and provided a basic cultural sequence, this publication became important and was followed by the next generation of local archaeologists.

In the Central Andes, North American ideas were becoming more and more influential through the publications of Bennett (1945) and Willey (1946; and see Willey & Sabloff 1980). Along with definitions of concepts such as component, phase and tradition (Willey & Phillips 1958), the essential recipes for the organization of culture-historical reconstruction of the South American pre-Columbian past were now in place, and are today still used in most parts of South America.

The North American tradition of research in the Central Andes (the *'peruvianistas'*), although diffuse in origin, crystallized in 1946 at a conference entitled 'Reappraisal of Peruvian Archaeology' (Schaedel & Shimada 1982). It was at this time that the Virú Valley Project was taking place, a multi-disciplinary project claimed to be an holistic study of the complete culture history of a Peruvian valley (Willey 1946). It also attempted to go further than culture historical reconstruction by emphasizing 'function and context', and by looking for causal generalizations (Schaedel & Shimada 1982).

The end of the Viru Valley Project came at more or less the same time as Julio Tello's death in 1947. His post was taken over by R.Carrion Cachot who followed a policy which limited foreign research in the country, and for a short period the work of foreign archaeologists, especially North Americans, declined drastically (Schaedel & Shimada 1982). Later on, however, various circumstances combined to bring a new and significantly more durable wave of North American scholars to Peru. M.Summer, with R.Schaedel, negotiated a long-term agreement between the Peruvian government and the Fulbright Commission of the United States—the 'Muelle-Fulbright phase' (Schaedel & Shimada 1982). This phase lasted from 1958 to 1968 and was characterized not only by a constant stream of North Americans but also by archaeologists from Japan and Germany. Their impact was financial and technical rather than theoretical (Izumi & Terada 1966; Izumi & Terada 1972; Burger 1989; Härke 1995; Tsude 1995).

In Peru, the first Misión Científica Española en Hispanoamérica was set up in 1967 and, under the direction of J.Alcina Franch, a group of Spanish archaeologists worked for several years in the Chinchero area. Alcina Franch's

main interest was to test his Atlantic model for interoceanic connections, on the basis of a long-distance diffusionist model (Alcina Franch 1972). Despite more archaeological fieldwork by Germans, Japanese and North Americans, only the United States had any real theoretical influence.

The period after World War II witnessed important political changes in Colombia. The Liberal Party lost power due to internal division and, as a result, the conservative M.Ospina Perez was elected to office in 1946. He developed a moderate policy and in some respects was keen to share part of his power with the Liberals. Just before the end of World War II, Rivet left Colombia (having been given an appointment by the French Provisional Government of Charles de Gaulle), leaving behind him his creation, the Instituto Etnológico Nacional, and a well-organized group of disciples, as well as five European professors (Chaves Chamorro 1986). These people helped to consolidate the Instituto and, in the years that followed, determined the course of Colombian anthropology. It is interesting that, in 1944, when France was still trying to rise up from its ashes, the French Provisional Government provided funds to enable all the scholars working in the Instituto to undertake fieldwork.

Theoretical and political structures took on different forms in Colombian anthropology. A strong indigenista movement emerged and a private institution, the Instituto Indígena Colombiano, became active in trying to improve the situation of Colombian indigenous peoples; scholars working in the Instituto Etnológico were becoming acquainted with a variety of new influences including British functionalism, as well as the works of North American scholars such as R.Benedict, M.Mead, R.Linton and A.Kroeber (Chaves Chamorro 1986). Meanwhile, the Conservative Party was becoming stronger and the government was lurching towards the right. When L.Gomez became President in 1950 he destroyed the most important achievements of the previous liberal governments. His blatantly racist and anachronistic ideas permeated all levels of his political actions, and his anti-Indian and anti-black sentiments were clear to all. For him, the only way forward for Colombia was by emphasizing its Hispanic origins, and by systematically negating the population's indigenous and African roots.

Within academia the result was persecution, resulting in seven scholars abandoning the Instituto Etnológico Nacional, some of whom were helped by the Guggenheim Foundation to spend one or two years in the United States. Social anthropologists were the first to be marginalized in this way; archaeologists were the last—'el indio muerto pone menos problemas que el indio vivo' (a dead Indian poses fewer problems than a living Indian) (Chaves Chamorro 1986:168). The same pattern was to be repeated twenty years later under military governments in the Southern Cone.

Gerardo Reichel-Dolmatoff and Alicia Dussan were able to continue their work, first in the 1940s when they were based at the Instituto Etnológico Nacional and then, when Reichel-Dolmatoff became its Director, at the Instituto Etnológico del Magdalena on the Caribbean coast. During this time they not only carried out pioneering archaeological research at early pottery sites, but Reichel-

Dolmatoff also produced a monograph on the Kogi of the Sierra Nevada de Santa Marta (Reichel-Dolmatoff 1950; Reichel-Dolmatoff & Dussan 1951; Reichel-Dolmatoff 1965). His systematic use of ethnographic analogy to interpret the archaeological record was marred by diffusionism, although not in such an extreme form as those of his predecessor Pérez de Barrada, or of Rivet (Gnecco n.d.). Reichel-Domatoff's influence on several generations of Colombian anthropologists remains significant today.

Neither Reichel-Dolmatoff nor Dussan joined those anthropologists who attempted to stand up to the Gomez government. Their attitude is bitterly remembered by a former colleague at the Instituto Etnológico:

> Reichel-Dolmatoff, a stranger to the political arena, was closer to the conservative party than to the liberals, with a strong aristocratic outlook. He managed to remain untouched by the political debates which broke out in Colombia at that time and his work was free of interference, nor was he affected by the political changes taking place since 1948…. His archaeological and ethnographic research paid no attention to the exploitation of the Indians, nor to land rights, nor to matters of wider educational import.
>
> (Chaves Chamorro 1986:188)

In 1953, the dictatorial style of L.Gomez, as well as internal contradictions within the Conservative Party, fuelled the military coup of Rojas Pinilla. A new era in the political life of Colombia started, and the first guerilla groups appeared.

The 1940s were also a time when North American archaeological influence focused its attention on the lowlands of South America, especially through the activities of Clifford Evans and Betty Meggers in Brazil (see Funari 1995). They undertook their first Brazilian fieldwork in 1948 (Meggers 1992a) and, between 1952 and 1953, they also undertook intensive fieldwork in British Guyana and Venezuela. In 1954, as a result of an invitation from Emilio Estrada, Evans and Meggers began research in two regions of Ecuador: the Napo basin and the Guayas coast. Subsequently, they also visited almost all other South American countries. Their overall aim was to elaborate a chronological and spatial framework for the low-lands of Latin America, and to investigate the peopling of these territories; their main analytical tool was Ford's technique of ceramic analysis (Ford 1962).

Meggers and Evans popularized their approach throughout South America, mainly through the free, wide distribution of the Spanish translation of their handbook (1969), *Como interpretar el lenguaje de los tiestos* (How to interpret the language of sherds). The other part of their approach was to obtain a large number of carbon–14 determinations, both from their own sites and also from sites excavated by local archaeologists. As a result, they and their South American associates were able to arrange the archaeological material of the lowlands into a complex system of traditions and phases. They then equated the

concept of 'series' developed by the North American archaeologist I.Rouse for Venezuela and the Antilles with their own 'traditions', as applied in Brazil (see Funari, Ch. 10, this volume), thus providing a 'common denominator for discussing ceramic distributions' (Meggers & Evans 1978:548).

Meggers and Evans were also very active in developing training programmes and workshops, both in the United States and in various South American countries. In this way they not only created a close-knit group of South American collaborators but they also facilitated major research grants. After 1964, the Smithsonian created its own research fund which was widely used to support several programmes such as the Palaeo-Indian programme. Despite the fact that most of the leaders of the Latin American 'Social Archaeology School' belonged to the group closely associated with them, Evans and Meggers have recently been criticized as having created a kind of feud which was 'the product of a political battle for territorial control of the archaeology of the area' (Roosevelt 1991:105). This debate has recently become openly political, and comments are no longer concerned only with 'purely academic' matters (see Funari 1991; Roosevelt 1991; Meggers 1992a; Meggers 1992b).

In Ecuador, post-war archaeology was shaped by cooperative research undertaken by Estrada and the Meggers-Evans team. As opposed to Jijón y Caamaño, Estrada was a successful businessman from the coastal city of Guayaquil, who 'was more interested in world trade and international yachting than in the archives of the Indies' (Collier 1982:8). Not surprisingly, therefore, he and his North American colleagues postulated a Jomon (Japanese) origin for the Valdivia culture, arguing that it must have arrived on the Ecuadorian coast as a result of long-distance maritime journeys. Although he died young, Estrada's work—which made full use of the recently invented radiocarbon dating technique as well as stratigraphic and seriational methods—together with that of Meggers and Evans, stimulated Ecuadorian archaeology, especially on the coast.

During the 1950s another North American scholar, Donald Lathrap, commenced research on the banks of the Peruvian Amazon, and came into personal conflict with Meggers and Evans. Lathrap proposed the tropical forest as the origin of the Valdivia culture, and stressed the importance of the Amazonian lowlands for ancient Andean civilizations (Lathrap 1973; Lathrap 1975). Less influential than his opponents, Lathrap criticized them for neglecting palaeo-dietary and stratigraphic studies, but he, too, regarded 'diffusion, migration and invasion as the main processes of prehistoric cultural change. Their main interpretive difference lay in the direction and timing of such movements' (Roosevelt 1991:107). Lathrap concentrated his research in Ecuador and Peru and influenced the archaeologists of the region. As a result, in the early 1980s a group of his North American students, along with a few exiled Argentine anthropologists, began teaching in the newly created Escuela de Arqueología de la Escuela Superior Politécnica del Litoral (ESPOL) (Alvarez 1986). This institution was founded by the Ecuadorian archaeologist Jorge Marcos (1986),

who had previously obtained his doctorate in Urbana, Illinois, under Lathrap's direction.

In Venezuela, local archaeologist R.Requena, who had already published the first extensive report on the archaeology of the Lake Valencia area during the early 1930s, was instrumental in bringing three North American archaeologists to Venezuela during the war: W.Bennett, A.Kidder II and C.Osgood (Cruxent & Rouse 1958). Later, Osgood was joined by Howard, and together they carried out a wider systematic survey of the country, laying the foundations for a very distinctively American culture-historical reconstruction. Just after World War II, another North American archaeologist, Irving Rouse, visited the country and a few years later started excavating with J.Cruxent, who was not only a prominent scholar but occupied important positions in the Museo Nacional de Venezuela and in the Universidad Central in Caracas. In 1958, Cruxent and Rouse published a complete report of the archaeology of Venezuela: 'a detailed chronology for Venezuela, in order to provide a systematic basis for organizing and interpreting the archaeological material. This chronology consists of a series of areas and periods' (Cruxent & Rouse 1958:12). Their main disagreement with Steward (1949) was with his conclusion that there had been a single centre in the Andes from which people and cultural influence spread down into Venezuela, resulting in the rise of cultural achievement. Cruxent and Rouse (1958:2) concluded, instead, 'that people and cultural influence of very distinct origins and quite different ages have come into our area, as much from the south as from the east and west…' Their 1958 monograph, with its closely argued definitions of sets of units (eg., style, complex, series) to organize archaeological material, was a landmark in the archaeology of Venezuela, leaving its mark upon subsequent research in the country. This intensive research carried out by North Americans also paved the way for Venezuelan archaeologists to study for their postgraduate degrees in the United States (e.g., Wagner 1967, based on fieldwork in the Carache area in western Venezuela).

During the early 1950s North American input was also felt in Chile, where the Director of the Universidad Nacional de Chile had personally encouraged the US State Department to send Richard Schaedel there to set up a Department of Anthropology in the university. Schaedel arrived in 1953 and his organization of the department reflected North American academic structures in all disciplines, including archaeology. He also formed the first group of Chilean professional archaeologists and carried out fieldwork with them between Arica and La Serena in the north (Munizaga 1991). A few years later Schaedel and his Chilean associates published a report on the current state of Chilean archaeology (Schaedel 1957).

Meanwhile a somewhat different shift took place in Argentina. After a short period of military government, 'semi-democratic' elections brought an American-style regime to the country, led by A.Frondizi. Under this government the discipline of anthropology (covering both cultural anthropology and archaeology) was created and given official status in 1958 in the universities of

La Plata and Buenos Aires. Although the professors that headed these departments had different orientations, it was the Vienna School that was still the dominant force (see Politis 1992). In this new atmosphere, A.R.González began to teach at La Plata University, having returned from the United States a few years earlier with a doctorate from Columbia University. Already influenced by the ecological ideas of J.Steward, and by the cultural-historical North American approach, he was one of the first scholars to spread Childe's ideas in Argentina. He worked first in the Central Hills at the famous site of Intihuasi (González 1960), where he proposed the first hunter-gatherer sequence, and later in the Northwest (González 1963), and his work introduced alternative views to subsequent generations of graduates. The open intellectual atmosphere, and the North American orientation which the government promoted, also had an impact on the world of archaeology. New ideas appeared, research centres were created or expanded, and the discipline held out possibilities for a professional career. Social sciences were viewed as a tool of development, and the model to pursue was that of the United States.

Once again, Frondizi's government was brought to an end by an army coup in 1961, but after a short period elections were again held (the Peronista party was still banned). In spite of receiving a low percentage of the votes the Radical Party won the election. Illia became President, and his government adopted a type of social-democratic approach, through which the universities regained their autonomy and new posts again became available in all disciplines, including archaeology, which benefited greatly from the democratic, scientific and educational policies of this administration. Moreover, the President personally supported the International Congress of Americanists and made it a central focus in the celebrations marking 150 years of national independence. The continuing academic freedom gave rise to new ideas and approaches in the field of archaeology. Theoretically speaking, Buenos Aires University still followed the Vienna School, since Menghin and his closest students, such as M.Bórmida, were still teaching there, but other universities, such as La Plata, Rosario and Córdoba, were exploring neo-evolutionism, North American culture-history and culture-ecological research programmes. In these universities bibliographies frequently included books or papers by Gordon Childe, Julian Steward, Leslie White and Gordon Willey. Anglo-American influences were significant and investigations carried out in the Northwest (where the archaeology is closely related to that of the Central Andes) reflected these approaches, although the investigation of the hunter-gatherers of the pampas and Patagonia was still in the hands of Austro-German diffusionists.

During the 1960s links between Argentinian and Spanish archaeologists emerged, as a legacy from the influence of the Vienna School scholars on the archaeology of Spain during the previous decades. As a result, several Argentinian archaeologists (e.g., Pedro Krapovickas, Antonio Austral and Mario Cigliano), although not necessarily associated with the Vienna School, spent some time in Spain. At the same time, Spanish journals were keen to publish articles

and monographs by Argentinian—or by Argentina-based European—archaeologists (e.g., Bórmida 1969, Cigliano 1966). In spite of this, there was no Spanish theoretical or methodological influence on the Argentinian archaeological community.

Unfortunately, the Illia government was too good to last, and was abruptly ousted by a new military coup in 1966. The new dictators, prepared to perpetuate their control *ad infinitum,* drove the country towards a North American-dependent economy. A number of scholars emigrated, including a few anthropologists, while others were made to resign their positions. Most of these were assisted by the National Council of Investigation (CONICET), a national institution that retained a certain amount of autonomy because of the international prestige of its advisory board. During this period of military rule a distinct political and scientific division emerged between the two main universities, which were also the two main research centres. The University of Buenos Aires was still strongly dominated by followers of the Vienna School. Theoretical alternatives were not available and only a few French methodological procedures (such as François Bordes' lithic typology) were able to penetrate this monolithic framework. In La Plata, archaeology became more and more culture-historically and ecologically oriented. Meanwhile, North American research in the Andes of Peru and Bolivia was providing the train to which Argentine archaeologists could hitch their wagon.

THE 1970S, ARCHAEOLOGY AND MILITARISM

The 1970s brought an era of military governments in South America. Although some countries, such as Argentina, had previously had a tradition of anti-democratic assumptions of power, it was during this time that right-oriented upper-class sections of South American societies reacted against the spread of left-wing ideas in the continent. Unable to gain power through democratic elections, and threatened by increasing guerrilla activity, they allied themselves with the military in order to gain control. While the military provided the force needed to suppress popular resistance, the section of upper-class society behind the military provided the foundation for conservative politics. The only exception was Peru, whose military government had a completely different orientation, since the military coup of 1968 was aimed at establishing 'state socialism' and producing revolutionary changes in the country (e.g., agrarian reform). In other countries, such as Argentina, Uruguay, Chile, Ecuador, Brazil and Bolivia, authoritarianism and repression characterized the military governments, which became highly dependent on the United States and exercised various kinds of violence. Only Colombia and Venezuela were able to survive this decade with democratic governments and maintain an atmosphere of freedom.

The 1960s witnessed the emergence of a strong national archaeological tradition in Peru. Under Luis Lumbreras, director of the Museo Nacional, a programme of archaeology was developed at the Universidad de San Marcos,

under the direction of Ramiro Matos. At the same time, the Universidad de San Marcos received the support of North American functionalist and neo-evolutionist oriented archaeologists, in the form of on-the-job training (Schaedel & Shimada 1982).

In the late 1960s a nationalistic military government took office under General Velasco Alvarado, under whose policies some North American archaeologists encountered difficulties after completing their research projects (Burger 1989). This Peruvian situation caused an expansion of North American research in Ecuador. When Velasco Alvarado lost power and was replaced by General Morales Bermúdez, the participation of foreign scholars was again promoted and for the next eight years North American archaeological research had a high profile. During the years 1977–83 'an average of 23 projects were authorized annually by the INC [Instituto Nacional de Cultura]; over two thirds of these were directed or co-directed by foreign scholars' (Burger 1989:43). The great majority of these came from North America, selecting project locations and research objectives based purely on academic criteria. During this period an important Spanish project in the Chinchero area, near Cuzco, was also launched under the direction of J.Alcina Franch.

In Ecuador, the military government of President Velasco Ibarra , who had been in power since 1963, ended abruptly with another military coup. The new government was directed by a junta headed by Rodríguez Lara. During this period there was an increasingly large influx of North American scholars, who developed research programmes in the three main regions of the country: the coast, the highlands and the tropical rain forest. In the coastal region, research carried out by Lathrap, Marcos and others, concentrated on the emergence of social complexity, mainly through the study of the Valdivia tradition. In the highlands, archaeological studies of hunter-gatherers were initiated by M.A.Carluci (1960, 1961), an Argentinian from the Universidad Central de Quito, and later, R.Bell (1960) and W.Mayer-Oakes (1963) from the University of Oklahoma became deeply involved in the area. These investigations, which focused on early human occupations and lithic studies, lasted for almost three decades and involved the local archaeologist Ernesto Salazar, a former Ecuadorian student of François Franf çois Bordes, who was registered in the doctorate programme of the University of Oklahoma (Mayer-Oakes 1986).

During this period European researchers came to Ecuador, bringing a variety of theoretical and methodological approaches (e.g., H.Bischoff from Germany, W.Bray from the United Kingdom). In 1970, J.Alcina Franch and M.Rivera set up a long-term research project in Esmeralda, a project which had a 'carácter multinterpretativo—historicista y ecológico cultural' (Alcina Franch 1972:40). In 1976, a handbook of Ecuadorian archaeology, *Ecuador prehistórico,* was published by Pedro Porras (Porras 1984, 2nd edition), who followed the Meggers-Evans approach. Like several other South American archaeologists, Porras had spent a year in the Smithsonian receiving training in pottery seriation (Meggers 1992a).

In Argentina the political situation underwent dramatic and rapid changes. Faced by increasing guerrilla activity and general popular dissatisfaction, the regime was forced to hold free elections in 1972, which were won by the Peronista party. H.Campora, who became President, was supported by the Peronista guerrillas and the left wing of the party during his short period in office (Campora had to resign after a few tumultuous months to allow Perón to be elected President for the third time). During the brief Campora government, Marxism spread through the sciences, especially the humanities. In social anthropology 'dialectical materialism' was seen as the approach necessary to understand present and past societies. In the field of archaeology, the ideas of Gordon Childe were once again taken seriously, especially those represented by his *What Happened in History?* and *Social Evolution* (on the basis of which two books, American archaeologists accused Childe of being a typical evolutionist (Trigger 1989)). Angel Palerm's ideas about the 'Asiatic mode of production' in Meso-America (1973) were intensively discussed, although attempts to find parallel developments in South America failed. In spite of all this, no profound changes occurred in archaeology, and Marxism provided only a cosmetic cover for a few culture-historically oriented research projects. Vienna School followers lost status and support in the University of Buenos Aires and for a short time a few of them were persecuted (students tried to subject them to popular 'academic' trials in an attempt to get them expelled from the university). During this period the culture-historical approach, and Steward's ecological approach, constituted the main alternative paradigms.

In the early 1970s, 'Argentine socialism' and the recovery of 'national identity' emerged as primary goals in Argentina. During this period the Argentine government organized an archaeological exhibition in Cuba as part of an integration programme with that country. In 1974 the Third National Archaeological Congress took place, characterized by the active involvement of students not only in archaeology but also in potitical issues. A few left-oriented Latin American archaeologists, such as José L.Lorenzo from Mexico and Lautaro Nuñez from Chile, also participated in the meeting, adding an international flavour to the revolutionary atmosphere which surrounded the Congress. The period during which these political claims emerged was too short, however, to allow archaeology to catch up.

Perón took office for a third time in 1973, and his policies quickly moved to the right. He condemned the guerrillas, while fascist elements emerged in the Peronista ideology, threatening the social sciences and paving the way for subsequent persecution. When Perón died, a year later, he was succeeded by the Vice-President, Isabel (his third wife). Her administration was fundamentally weak and she was unable to cope with the violent confrontation between the two wings within the party. Moving further and further to the right, with the support of para-military forces, her government drove the country towards a very difficult political and economic situation, culminating in the 1976 coup. The regime that took over in 1976 proved to be more violent than any previous one,

and was responsible for thousands of killings and for the development of sophisticated methods of torture and repression. Left-oriented social scientists had to go into exile in order to avoid being caught and tortured or killed by the army. Among those who were forced to flee the country were several archaeologists (mainly the first generation of A.R.González' students), while several archaeology students became *'desaparecidos'*. At least five young archaeologists went to other South American countries, following the same path that their Chilean colleagues had taken a few years earlier. A.R.González himself was dismissed from the university (although he maintained his position in CONICET) and support in the form of grants was discontinued. Several departments of anthropology were closed, and those that survived changed their curricula. By this time research in the Northwest had only a low profile. The surviving archaeologists, who turned their backs on Austro-German diffusionism, were oriented towards culture history, although they also discussed early ecological-systemic approaches (especially those by Kent Flannery and David Clarke).

In 1970, a significant political event occurred in Chile. The Unidad Popular party, with Salvador Allende as President, won the election and ushered in a short period of socialist government. Anthropologists were able to engage in open Marxist debate, but this debate was unable to transform the theoretical structure of Chilean archaeology. In 1973 a violent military coup, led by General Augusto Pinochet, overthrew the government of Allende, killing the President and resulting in the exodus of a large number of people. Archaeologists with strong Marxist convictions, such as Julio Montané and Felipe Bate, were let into Mexico and never returned to live in Chile. It was during this period that a few North American archaeologists were able to work in the country, including some students of Schaedel and Murra. These young scholars helped to change the orientation of Chilean archaeology from culture-historical reconstruction to an ecological-systemic approach. At a meeting held in the early 1980s, Primeras Jornadas de Arqueología y Ciencia, most Chilean archaeologists viewed the so-called 'New Archaeology' as the most fruitful approach to an understanding of past societies and the formulation of laws about human behaviour (see the debate in Arqueología y Ciencia Primeras Jornadas 1983). For them, archaeology was basically a social science, and the goal to which they aspired was 'scientific archaeology'. During this period one of Chile's prestigious senior scholars, H.Niemeyer, complained that most Chilean archaeologists had been self-trained, and his opinion was that the way to improve the quality of archaeology in the country was to bring in foreign professors (Arqueología y Ciencia Primeras Jornadas 1983:18).

In Uruguay, the development of archaeology took a different route during the period of military rule in the 1970s. The country had been influenced by the Vienna School, albeit second-hand, through Menghin and followers of the Universidad de Buenos Aires, and this led Uruguayan archaeologists to define a local derivation of the Austro-German diffusionism approach known as the

'Escuela de Buenos Aires' (Cabrera Pérez & Curbello 1992). This Vienna School approach shaped the work of the pioneer archaeologist Antonio Taddei, who collaborated with Marcelo Bórmida, an Italian-Argentine disciple of Menghin. Both researchers explained the past of the country in terms of the influx of successive waves of populations, each coming from different cultural 'circles'. During the rule of its military government, Uruguay received a French scientific team, sponsored by Unesco, to carry out rescue archaeology in the area about to be flooded by the Salto Grande dam. This project prompted the creation of a programme of archaeology at the Universidad de la República (Montevideo), and many students were involved in intensive fieldwork organized by French archaeologists under the direction of the Brazilian (French-trained) Niède Guidon. The project also gave rise to some opportunities for young scholars to pursue postgraduate studies in France. More significantly, the nature of the project encouraged these young scholars to adopt a distinctive approach which still characterizes the current practice of archaeology in Uruguay today. This approach includes, among other things, a deep concern for the preservation of the national archaeological heritage. Currently, the main archaeological project in the country, carried out in the Department of Rocha, is sponsored by the 'Comisión de Patrimonio Histórico, Artístico y Cultural de la Nación' which, together with the Universidad de la República, employs the majority of archaeologists in the country.

CIVIL RULE AFTER THE MILITARY

During the 1980s South America witnessed the recovery of democracy and was able to celebrate rights lost in earlier decades: those of freedom and plurality. The military, unable to administer and control these countries, and faced by popular dissatisfaction and pressure, gave up. New democratic governments took office and, in the field of archaeology, new dialogues developed in which multiple voices could be heard.

In those countries which did not undergo a right-wing military coup, a set of ideas based on a deliberate Marxist orientation began to emerge during the 1970s, which later developed into a 'Latin American Social Archaeology School'. Several early Marxist papers had provided the basis for the development of this school, which recognized, in the works of Gordon Childe, the foundation for archaeology as a social science (Vargas & Sanoja n.d.). Among the early Marxist writings were those by Peruvian Emilio Choy, which deeply influenced young Peruvian archaeologists of the late 1950s and early 1960s. Tabio's and Rey's (1966) book on the prehistory of Cuba, and Chilean Marta Harnecker's theoretical essays on French structural Marxism, inspired the Latin American Marxists. During the early 1970s, books written by Peruvian Luis Lumbreras (1974), and Venezuelans Mario Sanoja and Iraida Vargas Arenas (1974), became the seminal papers of the school. In 1975, at the 'Reunión de Teotihuacán' in Mexico, the members of this school of Latin American Marxists

sought to establish a radical programme for archaeology (Lorenzo, Pérez Elías & García-Bárcena 1976; McGuire 1992).

There were several reasons why the main meetings, and most discussions of Latin American social archaeology, were held in Mexico. The country had a long tradition of taking in left-oriented, persecuted politicians, including Trotsky. It had also welcomed republican veterans from the Spanish Civil War, among whom were the anthropologists Pedro Armillas, Pedro Carrasco and Angel Palerm, who were eager to discuss the ideas of Childe, as well as general Marxist issues (Mirambell & Pérez Gollán 1989). Mexican governments were always ready to accept Marxism within the academic environment, and Mexican universities gave posts to exiled archaeologists from Chile and Argentina, after the military coups in those countries. Thus (and to a great extent thanks to the efforts of Childe's former student, José Lorenzo), José Pérez Gollán from Argentina, and Julio Montané and Felipe Bate from Chile, found, in the Instituto Nacional de Arqueología e Historia (INAH), a congenial environment in which to pursue their careers. Mexico then hosted a series of conferences, culminating in 1975 (and see above) with the 'Reunion de Teotihuacán' (Lorenzo, Pérez Elías & García-Bárcena 1976).

Peru also emerged as a centre of debate, after the 'Congreso del Hombre Andino' and the publication of the very influential writings of Luis Lumbreras, who strongly advocated the social practice of archaeology as a tool with which to fight for indigenous and oppressed peoples' rights. At this time the research objectives of the early Latin American social archaeologists were focused on two main goals: the conceptual discussion of historical materialism applied to archaeology, and the interpretation of archaeological data in the light of historical materialism (Vargas & Sanoja n.d.).

By the end of the 1970s a certain amount of unease began to surface as a result of the lack of resolution in the wider debate of Marxist theory, and some archaeologists, such as José Lorenzo and Angel Palerm, grew weary of the rhetoric (McGuire 1992). On the other hand, a group of Latin American social archaeologists, including Luis Lumbreras, Manuel Gándara, Mario Sanoja, Marcio Veloz Maggiolo, Iraida Vargas and Felipe Bate, formed the 'Grupo Oaxtepec'. Dissatisfied with Marxist debate of the 1960s and 1970s (e.g., Sanoja & Vargas 1978; Bate 1978; McGuire 1992), they claimed to be committed to the analysis of the historical process in order to explain the history of the peoples of Latin America (Veloz Maggiolo 1984). Their theoretical framework was historical materialism, while their methods were derived from dialectical materialism (Bate 1977). The Oaxtepec group 'strongly rejected French structural Marxism, especially its ideas that modes of production can exist as separate entities that come into articulation' (McGuire 1992:67). The group attempted to reformulate historical categories and to develop the existing theoretical potential of others, such as 'mode of life' and 'mode of work' (Vargas & Sanoja n.d.). They also proposed the use of concepts such as 'mode of production' and 'socio-economic formation', because they felt that categories derived from traditional

ethnology were not viable or adequate for the understanding of pre-class societies (Veloz Maggiolo 1984; Sanoja & Vargas Arenas 1992).

Perhaps surprisingly, this school has accepted 'Ford's Method' for pottery seriation not only as a way to order chronological sequences, but also as a source of clear dialectical data (Veloz Maggiolo 1984). 'Ford's Method' is thus claimed to be 'the most efficient method to enable clear inferences to be made about cultural patterns in tropical archaeology' (Veloz Maggiolo 1984:11). In this sense, paradoxically, the strong influence of Meggers and Evans remains evident.

Although the Latin American social archaeologists were well known outside Latin America (especially in the United Kingdom and Spain, and through the widely distributed *Boletín de Antropología Americana*), inside their own countries, and in the region as a whole, their ideas were not as influential as they might have appeared. In Venezuela and Peru, far from being a dominant paradigm, this school competes with culture-historical and adaptationist-orientated research programmes. In Argentina and Chile (where the military governments would have made any Marxist approach very difficult), other approaches are explored, especially by those involved in the archaeology of hunter-gatherers. There, for those who wish to break away from the Vienna School influence, or to move away from culture history, the only viable option was seen to be a neo-positivist, ecologic-systemic approach (without any claim for a dialectical relationship between the present and the past).

Although Colombia was not ruled by a military dictatorship, the political life of the country was far from quiet. In fact, in the last decade, as a result of continuing social tension and the confrontation between the guerrillas and the military forces, high levels of public violence have been the order of the day. In spite of this, there have been no dramatic political changes in the country. During the 1960s four Departments of Anthropology were created, two in Bogota, one in Medellín and one in Popayán, producing an increased number of professional local archaeologists. In 1971 the Fundación de Investigaciones Arqueológicas Nacionales (FIAN) of the Banco de la República was created (Gnecco n.d.), which became the main institution for sponsoring and disseminating archaeological research in Colombia. The new generation of young archaeologists still maintained some of the traditions originally nurtured by Rivet and Reichel-Dolmatoff, but in the last decade an ecologic-systemic approach has become more popular. Moreover, since doctorate programmes are not available in local universities, several graduate students have obtained post-graduate degrees in the United States. During the 1970s and 1980s several North American teams, in particular from the University of Pittsburgh, were admitted to the country to develop long-term research projects with local archaeologists, mainly from the Universidad de los Andes. Another foreign influence, firmly based in a culture-historical and ecological framework, stems from the British archaeologist, Warwick Bray, who, with local collaborators, has been carrying

out intensive research in the country in recent decades (Bray & Moseley 1976; Bray, Herrera & Schrimpff 1981; Bray, Herrera & Schrimpff 1983).

In Argentina, at the very end of 1983, the Radical Party won the national elections and Raúl Alfonsín became President. His administration, which could be characterized as a kind of social democracy, introduced a climate of academic freedom. Universities again became autonomous, curricula were radically changed (by the up-dating of themes, theory and methodology), directors in research institutes were replaced and, in general, plurality of ideas and alternative views were promoted. CONICET opened up a large number of new posts for young scholars, and brought back the archaeologists exiled in 1976. The Alfonsín government concentrated its cultural policies on developing the social sciences and on the democratization of knowledge. As a consequence, three new departments of archaeology were created in medium-sized universities, while three more were re-opened in other universities. New positions were made available and a large amount of funds was allocated to the consolidation of studies in the country.

The archaeology of Argentina during the mid-1980s was characterized by an expansion of investigations concerning pre-Hispanic hunter-gatherers in most regions of the country. Most of this research was carried out using an eclectic ecologic-systemic approach, basically because the so-called 'New Archaeology' was the more familiar school since it had been developed locally over the previous years. During this period, publications by Schiffer (1976), Binford (1977, 1978, 1981), Kirch (1980) and other North American processual archaeologists provided the theoretical foundations for changing research strategies and they stimulated much debate. The 'New Archaeology' also provided methodological tools for carrying out regional research and placing it on the agenda of international discussion. With the return of the exiled archaeologists interest in the Northwest, and in the archaeology of complex societies, was reinforced. These archaeologists began a long-term project in Catamarca, Tucumán and Jujuy, but free from strong theoretical influence from the North American ecological-systemic approach of the 1980s. Although some of them had been earlier involved in the Latin American 'Social Archaeology School', this approach is currently absent from their scientific orientation. Some concepts deriving from the Vienna School are still present in archaeological discussions about the pampas and Patagonia but this approach has lost part of its scientific and political influence. Instead, Argentine archaeology of the 1980s and 1990s is characterized by active and fruitful discussion between adherents of alternative viewpoints.

CONCLUSIONS

Although archaeology in Spanish South America has been characterized by a rather heterogeneous development, some similarities can be identified. First, in spite of the Spanish colonization of the continent and the existing Spanish

tradition there has been little if any Spanish impact on the development of archaeology and on the present theoretical structure of the discipline in South America. Spanish archaeologists have visited the continent and some have undertaken research (e.g., J.Pérez de Barrada in Colombia, J.Cruxent in Venezuela, S.Canals Frau in Argentina , J.Alcina Franch in Ecuador and Peru), or have written large textbooks (Alcina Franch 1965), but they have left no distinctive traces in South American archaeology. There is no Spanish footprint that can be recognized in the archaeology of the region. Despite the occasional Spanish fellowships for South American scholars, and despite receiving visiting professors from South America, this exchange has been neither systematic nor frequent. Even though some Argentine and Peruvian archaeologists have occasionally published in Spanish journals (e.g., *Ampurias, Trabajos de prehistoria, Revista de antropología americana*), the Spanish-South American dialogue can only be characterized as circumstantial and intermittent.

The reasons for the absence of theoretical and methodological Spanish influence in South America seems to be twofold. First, there has been no strong conceptual innovation or discussion originating from Spain; in this sense the Spaniards were consumers rather than producers. As stated by Alcina Franch (Alcina Franch 1975), archaeology in Spain between 1940 and 1970 was characterized by a lack of theoretical orientation and coherent research programmes as well as by unswerving adherence to historicist interpretation (see Vásquez Varela & Risch 1991). Furthermore, while the Franco regime (1939–75) held sway, Spain was virtually isolated from the debating of foreign theories, especially those of a Marxist orientation. During this time the culture-historical perspective was dominant and the main influences came from German archaeology, while Spanish archaeology continued to follow pre-war theoretical approaches (Díaz-Andreu 1993). Only in the last decade, as a consequence of the social and political changes in Spain, has it been possible to identify thoughtful debate in a contemporary context in that country (Vázquez Varela & Risch 1991). This debate involves not only processualist ideas, but also a variety of approaches such as ‘critical theory’, structuralism and Marxism. This set of stimulating ideas is only now beginning to surface in debates in South America.

Second, when archaeology became a scientific discipline in the continent, South America was no longer under Spanish political and economic control. Some aspects of twentieth-century Spanish intellectual life, such as literature and philosophy, certainly influenced South American societies but the impact of this was generally confined to the arts and humanities, and did not make itself felt within the social sciences.

It is probable that indirect Spanish influence came to the fore in Mexico and later affected the work of some Andean archaeologists. It was in Mexico that the few long-term Spanish projects in South America were concentrated, which promoted a certain amount of exchange between Mexican and Spanish archaeologists. Mexico had also taken in several Spanish Civil War veterans who left a mark on the anthropology and archaeology of the country (Mirambell &

Pérez Gollán 1989) by promoting two main trends. First, they set in motion discussion of Marxist ideas. Second, they helped spread Childe's ideas during a period of strong North American influence by discussing them and confirming their relevance in the context of South America. The papers written by Armillas and Lorenzo debating Childe's concept of a 'Neolithic Revolution', and its application to American sites, were widely discussed by Andean archaeologists. Although the path is difficult to trace clearly, the dialogue of the Spanish Republicans helped to give a social dimension to the local archaeology of Spanish South America and, along with other contributions (such as the Marxist orientation and the earlier *indigenista* movement), paved the way for the later development of the Latin American Social Archaeology School.

In the last decade attempts have been made by Spain to restore economic and intellectual links with its former colonies in the continent. Nowadays there is considerable Spanish investment, especially in the purchase of South American state companies which are in the process of being privatized. Along with this attempt a few collaborative projects have been inaugurated in the past decade, so as to take advantage of the large amount of money allocated by the Spanish government to celebrate the fifth centennial of the discovery of America. However, these projects have been very localized and the Spanish presence is noticeable only where fieldwork is being carried out. They have not yet influenced the conceptual framework of South American archaeology, nor have they promoted any new debate.

A second conclusion is that South American countries initially received some evolutionist ideas from Europe but later, as a result of the impact of theories deriving mainly from North America and secondly from the United Kingdom, Germany and Austria, fell into line with the world trend towards a more diffusionist and historical perspective. This reflects South America's position on the international stage under the political and economic dominance of the United Kingdom, France, Germany and the United States at the end of the last century. At the beginning of the century the United States strengthened its position in the continent and played a hegemonic role after World War II. This is reflected to some extent by the easy and sometimes uncritical adoption of theories and methods from the central powers during this time. It is also important to note that some of the most influential local archaeologists in the region, from J.Tello to A.R.González, received some of their training in the United States, which resulted in rapid diffusion of North American ideas into South America. Even the Latin American 'Social Archaeologists' were caught up in this trend and maintained ongoing affiliations with North American institutions. It is important also to note the continuing funding that has been allocated by North American agencies to support archaeological research in South America. Typical examples of this are the National Science Foundation, the National Geographic Magazine, the Guggenheim Foundation, the Wenner-Gren Foundation and the Fulbright Commission, which sponsor projects located in South America and support the training of local archaeologists in the United States. Although most of the

research funding is given to North Americans, some money also reaches South American scholars who adapt to North American standards. No other country in the world has provided and maintained such fundamental economic support since World War II.

Associated with such North American control in the region during the 1970s and 1980s there emerged an ecological-systemic approach in South America. In some countries such as Chile, Argentina, Brazil and Uruguay, US domination led to dependence on the military government. It was during this period that large foreign debts were accumulated, and the servicing of these debts has, and will, compromise future generations. Of course, the principal lending institutions are North American.

The 1970s and 1980s was a time of broad expansion of theoretical and methodological discussion in both the United States and the United Kingdom, although British debate was less influential for three main reasons. First, the discussion involved only a few British processual archaeologists (e.g., D.Clarke (1968, 1972), C.Renfrew (1982)) and was less intense in the United Kingdom during the 1970s. Second, British case studies and examples were generally not applicable in the South American context, while North American ones were more closely relevant. Third, the United Kingdom exercised little political and economic influence in South America at this time. Therefore, British intellectual achievements were less widely known in the region.

Those countries, such as Mexico, Peru, and to some extent Colombia, which did not suffer right-wing military coups and whose indigenous peoples' traditions were somewhat stronger, were less keen on the new ecological-systemic approach. Paradoxically, Mexico and Peru are two countries where North America has concentrated more research and resources and, as a consequence, the archaeology of both areas is better known than that of other parts of South America. In these two countries, archaeology is used as a means to dignify the pre-Columbian past and to promote nationality based on the pasts of the indigenous people. In both countries Childe's ideas provided the background for the development of Latin American 'Social Archaeology'.

There were also French influences on the development of archaeology in Spanish South America, though these are difficult to trace. During recent decades such theoretical approaches continued separately from Anglo-American trends, a situation which Audouze & Leroi-Gourhan (1981) called 'continental insularity'. During, and following, World War II French influences were brought to bear on Colombian archaeology both in the form of Rivet's diffusionism and Mauss's sociological approach. More recently, French influences have been apparent at three levels: technical, analytical and theoretical. At the technical level, French archaeologists introduced sophisticated and rigorous excavation techniques into the region, especially in the Andes (see, for example, Lavallée, Julien, Wheeler & Karlin (1985)), and in Colombia and Uruguay. At the level of analysis, François Bordes' (1950, 1961) widely followed typology of the European Palaeolithic was very influential among archaeologists working in

lithic studies. The Bordes also trained some South American students in Bordeaux. At the level of theory, French influences have been far more diffuse, deriving basically from the structuralist and semiotic interpretations put forward by Leroi-Gourhan (1965). In spite of the fact that France has had very little economic or political influence in South America during this century, the intellectual and political elites have constantly admired the French intellectual style. It is this tradition that has been mainly responsible for the diffusion of French views among South American archaeologists.

North American involvement, and to a lesser extent that of West European countries, can be seen from two contrasting perspectives. One is that the presence of North American researchers in South America has been an example of cultural imperialism, in which South America has served as a laboratory for the testing of ideas and methodologies, or as an appropriate venue for the training of North American students. Likewise, such involvement can be seen as an example of cultural imperialism by which American appropriation and manipulation of knowledge of the past has ignored the peoples' own traditional perceptions. The other perspective is a neutral one in which North American research is seen as the consequence of scientific interest, free of any political motive. From this perspective, North American involvement has had a positive connotation, permitting expanded knowledge of the archaeology of the region.

This issue has been raised by Ponce Sanguinés (1978), Lorenzo (1981), Trigger (1984), Bray & Glover (1987), Lowenthal (1990) and others, but debate still has a low profile and is not seen to be of central contemporary archaeological concern. However, if one agrees with Lowenthal that:

> In discovering, correcting, elaborating, inventing and celebrating their stories, competing groups struggle to validate present goals by appealing to continuity with, or inheritance from, ancestral and other precursors…In this search archaeologists form part of the cadre of historians, social scientists, and other scholars increasingly pressed to defend or resist claims to this or that interpretation of the past.
>
> (Lowenthal 1990:302)

then it becomes clear that the histories about the past of South America have political implications. In this case, they are coloured by a North American and European perspective and were designed, consciously or not, to satisfy the needs of western scholarships. Certainly, the agenda has not been set in South America. Research topics, objectives and methodologies have basically been produced in the United States and secondly in Europe. From there, they have been introduced into South America, and viewed as parameters for the scientific validation of local research. Standards regarding what is right or wrong, out of date or fashionable, methodologically correct or incorrect, are established outside South America.

European post-processual approaches (e.g., Kristiansen 1984a, Kristiansen 1984b; Hodder 1986; Shanks & Tilley 1987a; Shanks & Tilley 1987b,) are only just beginning to have any influence, due to their European origins outside the orbit of strong socio-political and intellectual influence from the United Kingdom or Scandinavia, and because the anti-positivist reaction to other archaeological approaches has already been exploited by the Latin American Marxists. Nevertheless, in Latin America there is now some interest regarding the use of the past by the dominant social classes, as well as questions about the way that archaeological knowledge is built up, and how subjectivity (derived from both personal and political sources) affects ways of interpreting the past. Debate is beginning to focus on the relationship between the development of regional archaeological traditions and the political and social contexts in each of the countries concerned.

Archaeology in Spanish South America has changed over the last hundred years, not only as a result of the transformation of the position of the continent within the world political arena, but also as a result of changes in the nature of the academic power structure. Härke (1995:48) makes a very telling point: ‘The young German scholar makes his reputation by following in the steps of his academic teacher, in marked contrast to the British system where the young scholar would attempt to make his reputation by demolishing his teachers.’

The South American situation is somewhere in between. A young South American archaeologist who criticizes the teacher very early on will be seen as ungrateful and eager to make a career based on the demolition of contributions by older colleagues. In such an event, the ‘old boy network’ would probably succeed in meting out punishment. Yet, if the teacher’s ideas are followed too closely, the accusation will be of being too conservative and out of date, and the academic community will not be supportive. Thus, it can be seen that change in theory and methodology in South American archaeology is based on the adoption or development of a new conceptual framework in a gradual way and, preferably, not too early in an archaeologist’s career.

So far in the history of South America there has been no such thing as a school of ‘indigenous archaeology’, if that implies a way of thinking and practising archaeology which has not been been derived from western archaeology. Most South American archaeologists continue to practise culture-historical reconstruction, elaborating empirical generalizations in their countries, or trying to apply neo-evolutionary and adaptative concepts, with a high degree of eclecticism, to their own specific research problems. South American archaeologists are also still trying to fill gaps of information, by constructing cultural sequences for large areas where archaeology only began very recently. South American archaeology has often been forced to pursue its research in unstable political situations, and very often against a background of an unpredictable academic situation. In such a context, the production of theory is usually seen as a ‘foreign country’, while daily practice is a means to survive and, at least, to keep some dreams alive.

NOTES

1 This chapter cannot attempt to cover all the countries of Spanish South America in the same amount of detail. My choice of examples to be discussed is arbitrary, based on the extent of my personal knowledge and the information I could access; this is the reason for the extended treatment given to Argentina, Colombia and Peru. I have also tried to discuss some events relevant to countries such as Venezuela and Chile, which are not treated in depth in this chapter. The main objective has been to single out some of the common elements in the development of archaeology in Spanish South America and to explain the traditions peculiar to some countries of the region.

2 He based his conceptual framework on the systematic organization given to the *Kulturhistorische Methode* by Graebner (1911) and on the set of papers written by the same author along with W.Schmidt and W.Foy in the journal *Anthropos* from Vienna and *Ethnologica* from Cologne. He also recognized the work of F.Ratzel and L.Frobenius as the predecessors of the Vienna School. In his theoretical approach, Imbelloni developed the idea of 'culture' as an abstract entity approached through ethnology, and discussed how to define 'cultures' after the examination of their 'sensitive products, that is to say, the mass of goods' (Imbelloni 1936:33).

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CHAPTER TEN
MIXED FEATURES OF ARCHAEOLOGICAL THEORY IN BRAZIL

PEDRO PAULO A.FUNARI

INTRODUCTION

Does archaeological theory exist in Brazil? The answer depends, of course, on the definition of the term 'theory'. Embree (1989:37) considered that 'historical archaeology in the broad signification includes meta-archaeology and how substantive research includes methodologies of data collection and analysis as well as the theorising of explanatory models'. The absence of explicit theoretical and/or methodological archaeological posts in Brazilian institutions (Faria 1989: 35) would suggest that there is a lack of theory in Brazilian archaeology, as in the archaeologies of other countries (Kotsakis 1991:69; Thomas 1995). Besides, it is still very common to dismiss interpretative papers as 'too theoretical' (MacDonald 1991:830; and see Cooney 1995). Theory is sometimes considered as:

> esoteric, subversive, anarchistic—something one should avoid as a matter of intellectual hygiene.
>
> (Harlan 1989:583)

Is it, however, possible to carry out archaeological fieldwork without theory? Is it possible to isolate action *(poesis)* from theory *(praxis)* (Croce n.d.: 41)? It is not difficult to conclude that there is no way of practising a scholarly discipline like archaeology without analytical frameworks. Theory is nothing more than 'viewing, contemplation', *theoria* meaning first the actual visual observation *(thea)* and as a consequence 'speculation', a 'set of ideas'. If we consider that 'history is not a set of facts about the past but, rather, a set of ideas about the past held in the present' (Wright & Mazel 1991:59), then it is clear there is no archaeological practice without a theoretical background. It is precisely in these terms that we can say that there is archaeological theory in Brazil, not as an open and explicit set of statements about the ontology of archaeological knowledge, but rather as an underlying hermeneutics useful for both fieldwork activities and reports, and papers in general. Disentangling this theoretical outlook from archaeological activities and discourses is, however, a daunting task considering

the multiple mediations connecting empirical activities and their supporting conceptual frameworks. Moreover, generalizations about scholarly disciplines require some boldness considering that new materials and findings of even small fields can undermine them and thus the best way of avoiding misinterpretations is to establish the criteria used to study the subject. This way it is possible to understand the proposed links between the explicit and the implicit in Brazilian archaeology.

Knowledge, as a social relation between people, and people and things (Tilley 1992:176), is a historical and political process of interpreting and acting in the world. Archaeology as an academic discipline is not free of social and political ties (Champion 1991:144) and archaeologists always work under pressure of questions raised by their own eras and societies (Burguière 1982:437). 'Any attempt to understand the present configuration of the discipline must therefore be grounded in a systematic and empirically detailed analysis of its past history and practice' (Pinsky 1989:91) and, in the process of this, the archaeologist needs to acknowledge the full extent of changing circumstances and standards in different historical periods (Burckhardt 1958:xi). All forms of archaeological practice and writing make contact with diverse social groups in different and changing times (La Capra 1992:439). This means that one must study, on the one hand, Brazilian history and society as a whole (and, in particular, Brazilian intellectual history) and, on the other, the international context of interaction with Brazilian society. As there has always been a wide variety of archaeological theories in Europe and North America, any attempt to identify European influences in Brazilian archaeological theory is a particularly difficult task. There is, however, at least one clear difference between North American and European archaeological thought: 'throughout Europe, archaeology's closest intellectual ties are with History' (Hodder 1991:10), while 'History, both as a discipline and as a methodology, has always been viewed as largely irrelevant to prehistoric archaeology in the United States' (Trigger 1989:19). As will be seen, Brazilian archaeology has swung between historical and anti-historical trends as a result of various internal and external factors. This chapter deals with the formative, pre-disciplinary, period of Brazilian archaeology (up to the 1950s), bringing into focus its theoretical development since its introduction as a scholarly activity in the last four decades.

EARLY EUROPEAN INFLUENCES

The European character of Brazilian elite culture is acknowledged by modern foreign (Hale 1989:225) and Brazilian (Melo 1974:247) scholars alike, and Brazilian intellectuals used to consider that Brazilian culture was first and foremost a European culture. One of the main national ideologists wrote in 1922, at the time of the first centennial of Brazilian Independence:

> We received the same heritage and civilizing assets (from the civilized world), the same culture, the same ideals, the same political and social institutions and we continue to breathe through the same cultural environment in which they breathe, and to brandish, the best we can, their aims, feelings and ideas.
>
> (Vianna 1956:40)

The 'civilized world' was European culture, from Classical to modern, from Christianity to bourgeois ideologies. In the beginning, Brazilian heritage was considered to be this 'civilized' heritage and Brazil's National Museum (Museu Nacional) paid particular attention to artefacts of 'civilized', foreign, origin (Funari 1991a:122–3). The Brazilian imperial house was European (Funari 1989: 60) and the interest of the Brazilian elite in prehistory and Indians was not for Brazilian reasons, but due to a clear desire to mimic European intellectual fashions. In keeping with this trend, Costa (1934:50) published a monograph in a supposedly European style but he was compelled to admit that 'in terms of Archaeology, the material is defective, papers are seldom published. Neto (1885), von Ihering (1895; 1904), Sampaio (1922) were the only pioneers who collected data and tried to sum up the available evidence'. Archaeology was not, however, a scholarly discipline in itself but a practical activity mainly linked to museums. The Museu Nacional do Rio de Janeiro, the Museu Histórico Nacional and the Museu Paulista were the driving forces behind archaeological activities, which were carried out not as scientific exercises but rather as patronal activities sponsored by museum directors (Schwarcz 1989:28–9). These archaeological activities were run by the museum directors as a *'cosa nostra'* (Da Matta 1991a: 5) and this patronage system proved to be very important to the later development of archaeological practice and theory in Brazil.

THE HUMANIST APPROACH AND THE DEVELOPMENT OF PATRONAGE WITHIN THE ARCHAEOLOGICAL ESTABLISHMENT

While empirical archaeological work was being carried out by museum directors under the auspices of elite patronal sponsorship, the leading Brazilian humanist, Paulo Duarte, was exiled for writing against the dictatorship in Brazil (1937–45). Duarte's (1946) passionate book, published on his return to' Brazil from exile, signalled the onset of the ethical commitment of Brazilian intellectuals in support of freedom, and against arbitrary rule by those in authority (Goulart 1990:154). It was Duarte who was to introduce scholarly archaeology into Brazil (De Blasis & Piedade 1991:167) and his role as promoter of the protection of Brazil's heritage was a clear break from the traditional pattern of archaeological practice. Duarte's was not an idiosyncratic humanism. On the contrary, it was because of his ethical approach to society that he was able to propose two revolutionary moves: the development of academic archaeological institutions,

and heritage protection. Museum directors, the traditional archaeologists of the Brazilian patronal social structure, would not have proposed these moves, which inevitably challenged their nepotistic rule, based upon social relationships rather than on merit and equal rights (Da Matta 1991b:399). Duarte's democratic outlook was foreign to Brazilian hierarchical society; his broad-minded French humanism, and his general pledge in support of human rights *(les droits de l'homme)* were enough to lead to a break with longstanding arbitrary patronal practices. Far from being romantic, as is often maintained, Duarte's commitment enabled archaeology to aim at a social role in Brazil (Funari 1992:8).

The military intervention in 1964 (Cammack 1991:35) marked the beginning of a nightmarish period of persecution: 'Brazilians could no longer profess a different view without being considered as external enemies' (Rodrigues 1984: 226). Pervasive use of torture by the Brazilian military government (Ames 1988: 169) and political persecution and exile (Morel 1965:248), were followed by the reinforcement of patronage and clientele networks (Roniger 1987:75–6) in support of the authorities. Intellectuals were exiled and 'some of our best professors were summarily dismissed from their posts' (Holanda 1982:13). Soon after the 1964 military coup, an agreement was signed between the United States Agency for Inter-American Development and the Brazilian Education Ministry reorganizing the whole Brazilian university system (Sebe 1984:72), under the aegis of the 'National Security' ideology (Ortiz 1985:85). The United States' action was a result of the fact that 'throughout the [American] scholarly community, efforts proceeded simultaneously to mobilize the West for world-wide ideological struggle, while parading disinterested objectivity as one of the West's distinctive values and institutions' (Novick 1988:16; Klappenberg 1989:1014). This positivist approach was behind the activities of some American archaeologists linked to the Central Intelligence Agency and the State Department (after Roosevelt 1991:106) in Latin America. Physical violence and symbolic violence coalesced and, as Kiernan (1991:11) emphasized, 'murder squads sponsored by regimes in Latin America have been another addition to political science, a fresh extension of the golden rule of free enterprise and private profit'.

As early as October 1964, Clifford Evans and Betty Meggers had organized what they called 'an intensive seminar teaching archaeological theory, methodology, ceramic classification and interpretation' to twelve pupils from seven different Brazilian states (Evans 1967:7). Immediately after the seminar, Evans and Meggers spent the month of November 1964 travelling through eleven Brazilian states and visiting university presidents and museum directors. A naive positivism was at the heart of this archaeological team outlook. Meggers (1979: 13) taught and trained a generation of Brazilian practitioners under the banner of fact-finding objectivity: 'I hope [that people will understand] that truth is more interesting than fiction'. She explained the significance of pottery seriation in culture-historical reconstructions:

> When the National Archaeological Programme (PRONAPA) began, we believed that an archaeological phase was an abstraction without any ethnographic basis. Now, however, we believe that phases, defined in terms of sequential series, represent separate entities, while traditions, defined in terms of phases which share common features, represent tribal or linguistic entities.
>
> Meggers (1987:13)

Archaeology as an experimental science (Miller 1975:7) was interpreted as alien to historical concerns and as a clean break with the humanities in Brazil. This kind of empiricism ran counter to the humanist approach proposed by Paulo Duarte. Humanism, a theoretical, historical and non-positivist approach, was accused by empiricists of being alien to Brazilian culture. Humanism was interpreted as Marxism, and Duarte's liberal democratic ethos was misrepresented as Marxist ideology. R.Schwartz (1988:71) pointed out ironically that: 'when right-wing nationalists in 1964 denounced Marxism as foreign [to Brazilian culture], perhaps they assumed that Fascism was a Brazilian invention.'

The anti-historical empiricism imported from the United States was introduced into a society completely different from American society, in which empiricism, competition, individual rights, and capitalism inside and outside the scholarly world constituted a consistent cultural framework. Empiricism in Brazil served different purposes. The Brazilian social system is based on non-capitalist (Faoro 1976:736) principles, such as hierarchy (Da Matta 1980:16), patronage (Leal 1949:23; Telarolli 1977:16), nepotism (Da Matta 1991a:4), friendship, kinship and favour (S.Schwartz 1988:237). From colonial times, acquaintances (Pastore 1991:12), clientele, corporation ideology and paternalism have been key elements of Brazilian social life (Lara 1988:110): 'favour is our almost universal mediation' (Schwartz 1988:76). Vianna (1987:13) was inclined to define this as a feudal system. 'In Brazil, thanks to deep historical roots, appointees are the rulers: people in power appoint relatives and friends. Education, competence and quality are alien criteria to our culture of privilege' (Castro 1991:2). There is a clear imbalance between the capitalist, individualistic principles behind positivism in the United States and the same approach when applied within a social fabric based on non-egalitarian, patronal values. This is evident in the practice of archaeology in the two countries. The main aim of empirical fieldwork is to collect artefacts and then to classify them. This approach equates museums and bank accounts: they must be filled with data (money) gathered by the scholar (or capitalist). The evidence collected by archaeologists must then be classified and transformed into facts and figures (cf. Shor 1986:422). This is what empiricists aim to achieve in the United States and they may be quite successful in their own terms. However, this is not the case in Brazil. The aim of spreading fieldworkers throughout the country, collecting artefacts in large numbers, storing them in lots of museums, constituting corpora

to be finally classified as raw data was largely ignored by Brazilian empirical archaeologists. Why?

From 1960 onwards Brazilians were being trained as fieldworkers by American positivists under the banner of non-historical ecological determinism.

> Their excavation and analysis methods mixed materials from different periods, artificially compressing the archaeological sequence. This North American approach, however, strongly influenced the Brazilian scholars thanks to agreements between Brazilian and American institutions *and to the establishment of a whole network of colleagues and pupils*.
>
> (Roosevelt 1991:107: italics added)

This team of practitioners did not develop, as was to be the case elsewhere, as an 'academic fiefdom' (Levine 1992:218) but, in the patronal society of Brazil, and under direct autocratic rule by the military, this group was the only one to be legitimized. They then proceeded to persecute or hinder the activities of those people who disagreed with their empiricist ecological approach and with their politically despotic (Chaui 1992:6) organization and outlook. Duarte and others were expelled from university life and the archaeological establishment, which had been in the very process of its creation and development, was dominated by the group of Brazilian authoritiarian empiricists.

This group formed a close-knit group of South American collaborators (who called themselves *irmãos* ['brothers'], see Meggers 1992), controlling excavations, funds, publications, museum and university archaeological posts and, last but not least, limiting the spread of any different or foreign perspectives. Even American scholars who had different, historical, outlooks were systematically obstructed in their work. As the American archaeologist Roosevelt stressed,

> although a lot of scholars found evidences [in opposition to the non-historical ecological approach], people from the determinist school often did not allow the publication of dissonant findings, like 'too early datings' or complex prehistoric settlements.
>
> (Roosevelt 1991:107)

The constitution of a controlling gang thus explains why empiricism in Brazil was not able to attain its own goals of collecting data, establishing corpora and finally classifying material on a large scale. As is usually the case in authoritarian social systems, it was impossible for alternative discourses and practices to develop, and there was thus no need for the archaeologists in power to be effective even in their own epistemological terms.

> 'Naked force', as applied in ordinary police-states, aims only at the preservation of 'law and order'—that is, outward conformity—and it has

> neither the capacity nor the ambition to create a 'new man' enthusiastically supporting all the aims and ideals of the rulers…a truly totalitarian regime can enforce much more than merely passive consent.
>
> (Walicki 1991:95, referring to communist Poland)

In the case of Brazil, active support meant both suppressing different standpoints and creating a discourse, however incompetent in its own terms, which could be considered as referential description of reality.

For example, Schmitz, a leading patron and authority on Brazilian prehistory, published a comprehensive study on hunter-gatherers in Brazil with no reference whatsoever to social organization or culture, using basically excavation reports. He made no explicit reference to any theory or method, although he wrote with a loose ecological determinism in the background. Of the many paragraphs with only one phrase, one is particularly paradigmatic in this regard: 'In the stone industry we find a lot of polished axes' (Schmitz 1991:15). In Brazil, empiricism and ecological determinism did not result in a consistent and strong positivist science, collecting, publishing and classifying archaeological materials, as its hermeneutic basis would have suggested. Thanks to the authoritarian regime, it was possible for a group of people to reinstate patron/client practices with arbitrary power, using empiricism first and foremost as a justification for their rule (Fig. 10.1).

PLURALISM AND ITS THEORETICAL OFFSHOOTS

Neves (1988:245) acknowledged not long ago that 'in Brazil, save rare exceptions, we continue to carry out opportunistic surveys or unjustifiable excavations and Brazilian teaching institutions despicably assist in perpetuating the epistemological model still in place in Brazilian archaeology'. Most archaeological activities and publications continue to be merely descriptive (Scatamacchia 1984:198) but empiricism and non-theoretical emphases are common features also in contexts as disparate as France (Cleziou, Coudart, Demoule & Schnapp 1991:117; Olivier & Coudart 1995), Germany (Härke 1991: 198; Härke 1995) or former Czechoslovakia (Neustupny 1991:261). However, the loosening of authoritarian rule in Brazil made possible the emergence of a plurality of approaches. As any archaeological activity is a political act (Hodder 1990:278), there was a growing awareness of the political and ideological influence of archaeology in contemporary society (Myhre 1991:173). The openness in political life and the ensuing freedom in academia enabled the human and social sciences to develop different theoretical and methodological schools. 'In accordance with a plurality of views in an open society, there should be room for different accounts' (Baker 1990:59). Pluralism and the bolstering of different views also led to the mushrooming of alternative approaches in archaeology (Dommasnis 1990:30; Cohen 1991:19).

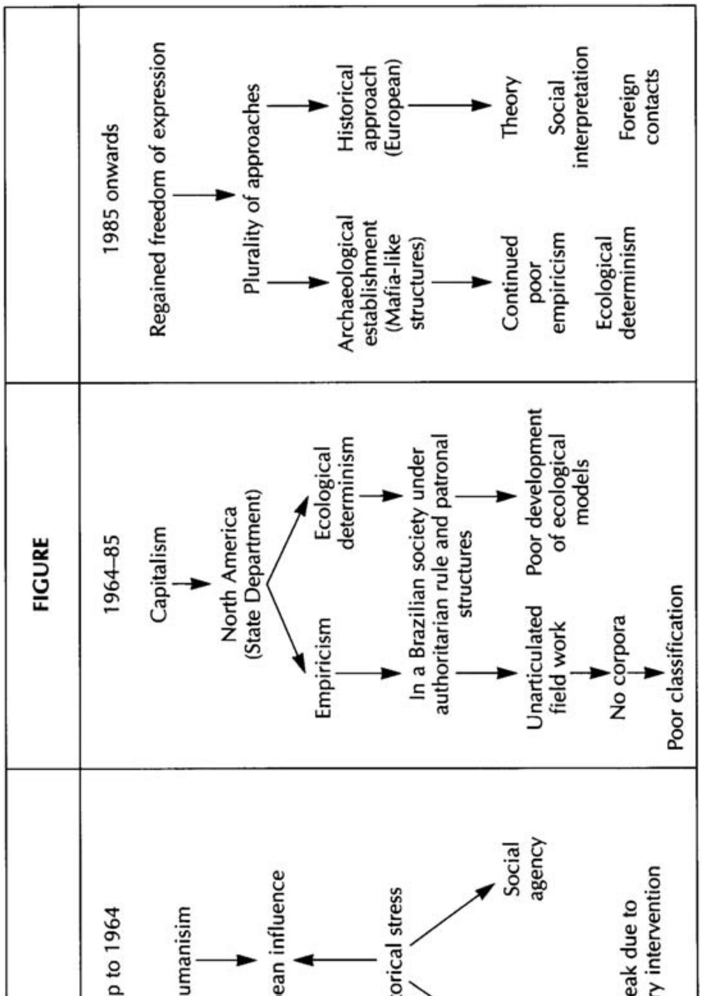


Figure 10.1 Authoritarian rule, anti-historical empiricism and ecological models in Brazilian archaeology

Classical archaeology played a special role in the theoretical discussion in Brazil for the same reasons. Classical archaeology is still considered as something separate from all other branches of archaeology in Europe (Härke 1991:192; Härke 1995): scholarly procedures and study are at the root of the

work by learned classical archaeologists. This can lead, in the case of developed countries, to a lack of theoretical concerns, but in the Third World scholarship means that a classical archaeologist needs to know not only Greek and Latin, but also various modern languages enabling him or her to confront different approaches in different cultural contexts and traditions. Besides, as classical archaeologists need to work abroad, they are in touch with different ideas and, last but not least, they can be more independent in relation to the empiricist archaeological establishment. The international context in which they work compels them to attain international standards of scholarship and this, in itself, is not a minor achievement. A case in point is the well-known Nouvelle Clio series: Brazilian prehistory was not entrusted to Brazilian scholars, but to two French archaeologists, Laming-Emperaire and Baudez (Leroi-Gourhan 1981). However, a whole chapter on Greek archaic civilization was written by Sarian, a Brazilian classical archaeologist (Sarian 1989:585–93). This theoretical overview of the subject is in fact paradigmatic in many ways: it is not mere description but, on the contrary, is an interpretative analysis. Her choice of references is also interesting, as she refers to 16 English, 7 French, 2 Italian and 1 German author. Such erudition and familiarity with foreign scholarship within classical archaeology has, however, been attacked by the empiricist archaeological establishment on the grounds that it is a scholarly field under foreign influence, assumed to be deleterious. It is, therefore, not surprising in the Brazilian context that the first theoretical and methodological introduction to archaeology by a Brazilian scholar (Funari 1988) was, in the words of Neves (1989:214): ‘ironically, written by a Classical archaeologist’ (but see Martín 1989:141).

An important archaeological theoretical movement within Latin America—the ‘Social Archaeology School’—fared badly in Brazil despite (or perhaps because of) the fact that it had, from the 1970s, ‘a real political and social commitment to the present historical circumstances’ (Marcen & Rich 1990:101). Luiz G.Lumbreras (the founding father of the School), among others (e.g., L.F.Bate), was dismissed during the period of military rule as a dangerous Marxist. His book (Lumbreras 1974) was published in Spanish and was thus easily readable by Brazilians; it is also available in Brazil, and is quoted, for example, in Funari’s bibliographical handbook (Funari 1988:84). However, it is seldom quoted in Brazil. In fact, the first paper by a Latin American ‘Social Archaeologist’ to be published in a Brazilian journal controlled by the archaeological establishment only appeared in 1990 (Zamora 1990).

Very recently there has been an upsurge of interest in theoretical archaeology in Brazil, mainly due to the zeal of a number of young students. Critical archaeology, presented as a critique of present ideology which is made to appear normative and ahistorical (Handsman & Leone 1989:119), together with a post-processual awareness of disciplinary subjectivity (Thomas 1990:67), are matters currently being discussed. Papers by young scholars on, for example, the effects of colonialism and nationalism on African archaeology (Rodrigues 1991) demonstrate a growing Brazilian interest in world archaeology and

archaeological theory. Such interest also reflects a growing awareness of foreign archaeological theoreticians.[1]

As a result of this preoccupation of a small but active minority of archaeologists, increasing interest has been shown in the study of the archaeologists' social commitment and their involvement with social issues (Shanks 1992:46). Following Stephen's (1989:267) and Hodder's (1991:10) ideas, more and more attention is being paid to subordinate groups and efforts made to support them in their struggle against marginalization. This explains the attention paid to the Indians living in Jesuit mission establishments (Kern 1989:112) and to the 'history of domination and resistance' (Leone 1986:431) in relation to exploitation from antiquity (Guarinello 1989) to colonial Brazil (Funari 1991b).

The growing importance of archaeological theory in Brazil can also be judged from the fact that for the first time papers by Brazilan authors are now being published on this subject. The first of these (Funari 1989) dealt with British critical or post-processual archaeological theory, and the latest, having reviewed theoretical approaches in archaeology, concludes that 'archaeology, following theoretical guidelines, must also be of practical importance for people's lives' (Kern 1991:14).

CONCLUSION: THE FUTURE ROLE OF THEORETICAL ARCHAEOLOGY IN BRAZIL

The development of archaeological theory, important as it may be in Europe and North America, is absolutely vital for the prospects of archaeology in Brazil. In the context of an archaeological establishment impervious to any change, by struggling actively even against those trying to follow empiricist international standards, theory plays a particularly crucial role in bringing up a new generation of archaeologists who dare to think, to interpret, to analyse and, last but not least, to challenge current ideas and practices. Despite the efforts of people in power within the archaeological establishment, be they museum directors, research council referees or other appointed officers, to control and to suppress dissenting voices their designs are doomed to failure in a pluralist society. Through reading in archaeological theory, some Brazilian archaeologists have been able to confront difficult and otherwise unsurmountable obstacles. Theoretical archaeology thus helps to transform Brazilian archaeology in a vitally important way and, if thinking about archaeology is not enough to change it, it is nonetheless a necessary step.

NOTE

1 The most popular authors consulted by young Brazilian archaeologists include Binford, Courbin, Deetz, Gardin, Hodder, Shanks, Tilley and Trigger.

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CHAPTER ELEVEN THEORETICAL UNDERPINNINGS OF PORTUGUESE ARCHAEOLOGY IN THE TWENTIETH CENTURY

VÍTOR OLIVEIRA and SUSANA OLIVEIRA JORGE

PORTUGUESE ARCHAEOLOGY IN THE 1900S

Throughout the twentieth century Portuguese archaeology has clearly reflected the country's peripheral and dependent situation in relation to the major 'producers' of scientific culture in Western Europe, especially France, but also Germany, the United Kingdom, and even neighbouring Spain. Despite the great effort at renovation since the late 1970s on the part of a college-educated generation (whose studies were usually completed abroad or supervised mainly by French or German researchers), archaeologists are still confronted by considerable misunderstanding from the public, and almost unbelievable indifference from most of the government towards this area of heritage and research.

The dictatorial regime which ruled Portugal from the 1920s to the mid-1970s was forthright in its hostility towards the social sciences. This led to a very restricted role for these sciences in the universities, both in research and teaching. Disdain for subjects such as sociology, anthropology and ethnology had an obvious negative effect on efforts to create an environment in which archaeology could develop, while archaeologists were becoming increasingly aware of the vast theoretical and methodological changes occurring abroad, which sooner or later would reveal themselves as vitally important to this area of enquiry. In addition, archaeology's institutional status, first as part of the so-called historical-philosophical courses and later just of history courses (taught in the faculties of letters), turned it into a mainly subsidiary subject alongside epigraphy, numismatics or palaeography. Portuguese archaeology—its main thrust being descriptive and empirical and its methodology improvised, intuitive, strongly individualistic and amateurish—echoed in this far-off fringe of Europe the positivism reigning in French and German archaeology (see chapters by Olivier and by Härke, this volume), which has been the main influence in Portugal via the more or less frequent presence of researchers from those countries (e.g., H.Breuil and the Leisners).

Most Portuguese archaeologists learn their archaeology in the field, through practice, following the example of the more experienced and, at home, selecting

materials and learning how to classify them. Often, as a last resort, consultation of some foreign textbooks allows them to construct a discourse proposing, in accordance with their own particular level of erudition or literary ability, a functional and chronological classification for the remains which they have excavated.

Meanwhile, the National Archaeological Museum in Lisbon, the former Museu Etnológico Dr Leite de Vasconcelos, under the long directorship of Manuel Heleno as its director, stands as a symbol of the stagnation described above. Despite being a central body of immense potential importance—and, furthermore, connected to the Faculty of Letters in Lisbon—the Museum has done nothing to implement the formation of a school, or the development of research to the level of work already being carried out all over Europe. On the contrary, the status in universities of prehistoric studies, which were introduced as a subject within general history courses during the 1957 reform of the education system, continued to be marginal—as they have for decades—since prehistory was taught either by specialists from other fields of research, or by self-evidently incompetent people.

Unlike other countries of the world (see Funari 1995, Politis 1995, Härke 1995 and Tsude 1995), the Portuguese authoritarian regime did not seek to take advantage of archaeology for any nationalistic purposes. It was really only after 25 April 1974 that so-called 'rescue archaeology' started to develop, and only in the 1980s that a centralized national archaeological survey and mapping programme became effective (and see Evans 1995).

In short, therefore, until very recently—and with only a very few exceptions (such as the example of Conímbriga, a Roman town and its 'monographic' site museum[1])—archaeology in Portugal has been essentially amateurish, even when taken up by organizations such as the Portuguese Geological Services.[2] The context for the practice of archaeology is one with only few Portuguese translations of methodological or general books, and a dearth of publications on archaeology issued by commercial publishers aimed at the general public. Archaeology in Portugal is neither registered as a profession, nor enshrined in law, and its status is therefore rather vague. In addition, the Portuguese archaeological community is very small, consisting of some two hundred people. In the 1970s, not more than half of these would have authored several publications, and very few of them would have published abroad. To put it in somewhat conventional terms, the Portuguese archaeological 'community' of today is made up of three main categories:

1 university teachers, who are therefore subject to the strict regulations of an academic career, with onerous deadlines and heavy teaching schedules, accompanied by job insecurity (given the universities' general drive towards economies of scale, and short-sighted economic policies leading them to make the most of existing staff);

2 archaeologists involved with heritage archaeology, more concerned with making inventories, with preservation, conservation and evaluation than with archaeological research;
3 scientists from the natural and so-called exact sciences who have demonstrated an interest in archaeology, have dedicated a good deal of time to it, or obtained degrees abroad in subjects such as physical anthropology, but whose professional situations are, in general, precariously difficult.

Ideally, of course, it is obvious that there should be no division or conflict between any of these categories. Clearly, successful intervention to study and/or protect a building or ruin requires research, and often scientific expertise. Yet, the preposterous division between the 'fundamental' and the 'applied' in practice forces some archaeologists to teach and research almost in a race against time so as to ensure the security of their university posts (which are considered a privilege because of their rarity), while others have to read and evaluate piles of bureaucratic files, usually without any facilities to study or evaluate the results of any actual interventions. In this latter group are those archaeologists who work for the Portuguese state body for the protection of cultural heritage (the Instituto Português do Património Cultural (IPPC), nowadays called the Instituto Português do Património Arquitectónico e Arqueológico (IPPAR)). Currently the whole state sector for culture is in crisis, particularly in the area of cultural heritage, and many of the jobs of these few archaeologists are acutely threatened.

In the 1980s there were some positive developments with regard to Portuguese archaeology on several fronts. This was in part due to the 1970s' admission into universities of teachers trained in new methodologies, and in part to the creation of the Regional Archaeological Services of the IPPC—services which have recently been reorganized. In addition, some excellent doctoral theses were produced and published, and some long-term research projects—based on previous elaborations of problems and questions and on teamwork—were designed. In the 1980s measures were under way to safeguard and value the heritage: laboratories, such as the carbon-14 one in Sacavém, had been installed, and the subject of archaeology made its appearance as an 'option' within university history courses. Overall there was increasing interest among young college students for research training and to be able to participate in excavations (although the absence of professional archaeological prospects led to a kind of 'natural selection' whereby only the 'fittest'—economically and/or psychologically—did not take up professions such as teaching).

Now, in 1994, there is a situation where vagueness rules the day. Those who wish to carry out substantial fieldwork, and who do not have a prosperous municipality to back them up, can only turn either to the National Institute for Scientific Research and Technology which supports the social sciences, or to programmes benefiting from European Union funds, such as those designed for the development of rural regions, or the development of tourism in border areas.

As far as archaeology is concerned in the Portugal of 1994, the most apt adjective to describe the current situation is 'asphyxiated'.

In the light of the above picture, it is not surprising that there has been no autonomous Portuguese production of theoretical archaeological writings. Only in the context of wide-ranging works, such as doctoral theses, have any recent authors attempted to define epistemological choices, and even these attempts have usually been undertaken within the framework of having to construct some methodological framework for their research endeavours.

THE EARLIER DEVELOPMENT OF ARCHAEOLOGY IN PORTUGAL

There are perhaps three main stages which can be identified in the leadup to the current situation of archaeology in Portugal.

The 'Origins' stage dates to the end of the 1850s and the beginning of the 1860s. In 1857, in Lisbon, the Geological Commission, now called Geological Services of Portugal, was founded resulting in geologists and physical anthropologists carrying out fieldwork (including excavation) in caves and shell middens, and collecting lithics. Only a few years later in 1863, and also in Lisbon, architects, concerned with the conservation of the built environment, contributed to the creation of what is now called the Portuguese Archaeologists' Association.

The 'Take-off' stage can be placed between 1870 and the late 1920s, up until the imposition of the dictatorship. The Geological Services continued their archaeologically related activities (their early prehistoric investigations under Carlos Ribeiro leading to the meeting in 1880 in Lisbon of the IX International Congress of Anthropology and Prehistoric Archaeology). Running parallel to these lines of enquiry were others which can be referred to as 'ethnological', involving those whose interests lay in the human sciences, and who were committed to establishing the roots of popular culture, seen as the basis for establishing a national identity. However, for the 'Take-off' to have occurred successfully it was essential that ethnography, social history and archaeology should have become recognized as interdependent studies, and this is exactly what was achieved by a few remarkable individuals.[3]

Physical anthropology was another of the important research developments of the time, especially developed by Mendes Corrêa of the University of Oporto. It was he who was responsible for the dynamic advance of the Anthropology Institute of the University of Oporto (after 1911, in the wake of republican university reform) and that of the Portuguese Society of Anthropology and Ethnology (in 1918), who first attempted a synthesis of Portuguese prehistory, included in a general history of the country (known as the Barcelos history) published in 1928.

Corrêa's article on 'Pre-Roman Lusitania' epitomizes the flavour of this second, 'Take-off', stage: positivism, nationalism, methodological descriptivism,

uni- or multi-linear evolutionism, and migration/diffusionist explanation. Ever since 1846, explicitly or not, there had existed a Portuguese concern to be able to proclaim Portugal a nation independent of the Spanish state. This second stage of Portuguese enquiry was concerned with such nationalistic sentiments—sentiments which were to endure.

The third, 'Stagnant', stage, could be said to have lasted from the 1930s to the end of the 1960s, corresponding to a time when the authoritarian regime was particularly strong. There was almost complete dependence on foreign researchers, exerting a direct influence through their published works, and Portuguese archaeology fell behind all other European countries in a quite spectacular way. Access to literature remained abysmal: even in Lisbon, photocopies of an unpublished text from the University of Coimbra were circulated as handout textbooks—the text dated from 1967 and, although it had no authorship, it was well known that Professor Jorge Alarcão had written it.

For whatever reasons, Portugal did not follow most Nazi and other Fascist regimes in developing archaeology as a means to legitimize the grandeur of the 'fatherland'. Therefore, there are many aspects which carry through into this stage from the preceding one. The Geological Services of Portugal kept on developing their survey and excavation activities (which were made easier through the elaboration of the country's geological map on the scale of 1:50,000—a task which still remains uncompleted). This was the institution that generally hosted foreign researchers.[4] In fact, during this 'stage', Portugal became an attractive field for other European archaeologists to carry out the researches which the Portuguese themselves could not afford to do.[5] Occasionally, some of these foreign enterprises became training schools for Portuguese archaeologists, and some of the foreign archaeologists concerned wrote highly influential papers about their discoveries and analyses, including Schubart (1975) on the so-called peninsular 'south-western Bronze Age'—even though his work was only published in the 1970s, and Anati (1968) on Iberian rock art. It is symptomatic of this 'stage' that the first noteworthy, though somewhat obscure, prehistory of Portugal should have been written by a British researcher (H.N.Savory) in 1968.

As with the Geological Services so, also, did the Anthropology Institute of Oporto continue to pursue its activities. However, the early death in 1933 of one of the best of Corrêa's students, R.de Serpa Pinto, together with the fact that the eminent ethnologist Jorge Dias[6] did not remain for any length of time at the Centre for Studies in Peninsular Ethnology, did not make the 'dialogue' between archaeology and cultural anthropology any easier.

It is also in this period that general works by Spanish prehistorians, such as Bosch Gimpera (1932), del Castillo (1947), Martinez Santa-Olalla (1946) and Pericot (1950) began to exert a profound influence. It is therefore hardly surprising that it was the principles of the culture-historical school—concerned with the definition of 'cultures' as discrete entities with a specific space- and time-span (equivalent to the peoples of historic ages)—which had become predominant in Portugal. Even today these same principles are fiercely defended

by some scholars, a defence which is symptomatic of an anachronistic and culturally provincial attitude (rather than of a reflective 'choice' between alternative interpretative paradigms—which would suggest the existence of an informed debate, a debate which, in practice and in essence, does not yet exist in Portugal).

Nevertheless, there were some positive aspects to this 'stage'; for instance, the development of scientifically based Roman archaeology at the University of Coimbra, in connection with the excavations at Conimbriga; the introduction by Serrão and Vicente in 1955 of stratigraphic observation, and of the grid method (created by Wheeler), in field archaeology at the prehistoric settlement of Parede (Cascais) and, in 1957, at the site of Olelas (Sintra); the translation into Portuguese of some of the major works of Gordon Childe; and, despite the fact that the archaeological 'community' was so small, the growing realization of the existence of an international community of archaeology.

Despite these positive aspects, and the concern of some (but not many) people about interpretation and methodological issues, the general perspectives within this third 'stage' were firmly and strictly empiricist, descriptive and, most of the time, impressionistic. There were few single-author quality monographs published by any Portuguese during this period. Typically, Viana (1962:67) could write:

Portuguese archaeology needs everything but theory.

Even a concern with synthesis would have been exceptional for the time. No doubt such stagnation was associated with the fact that archaeology at this moment was administered by the National Board of Education (Junta Nacional de Educação)—a bureaucratic council, lacking the necessary expertise to intervene in the cultural field. This Board depended for its functioning upon information which it had to derive from local deputies; in other words, it was dependent for its activities on an amateur and unremunerated structure which, in any case, could only cover a small part of the country.

Clearly, there have been close links between the above 'stages' and the dates of important political events, although it must be said that there are no exact coincidences. The crisis of the monarchy and the republican manoeuvres formed the background to one of the stages of development referred to above; the crisis in the colonial system and its downfall in 1974 to another.

BACK TO THE RECENT PAST, THE PRESENT AND, PERHAPS, THE FUTURE

The first big operations in 'rescue archaeology',[7] accompanied by the requisite financial support from the state or some international institution, were among the first signs that something was beginning to change in the 1970s. The influence of foreign archaeologists was also increasing, only now there were groups of young Portuguese trying to make the best of their presence. In 1972 the German Archaeological Institute of Lisbon was founded; from 1974 onwards, Roche—

until then 'settled' in the Geological Services—began a collaboration with the Faculty of Letters of Oporto, and directed excavations especially designed for the training of students; more recently English and American archaeologists have also been working in Portugal, in cooperation with local researchers. Meanwhile, the French Mission for Classical Archaeology continues to function.

National congresses, despite intervals between them, have begun to take place more frequently than before: in Coimbra in 1970, Oporto in 1973 and another in Faro in 1980 (the latter regrettably has not yet published its proceedings). In addition, all over the country, local or thematic meetings have multiplied. Initiatives derive from universities, municipalities or local groups and behind them are the dynamic spirits of a few committed people, committed in their efforts to call attention to themselves, to the themes they are interested in or to the area(s) they are studying.

It remains true to say that theoretical and epistemological questions have not generally been issues at such meetings. Their main concerns have been with more pragmatic and traditional themes. Nevertheless, there are now signs of possible change. Recently, in 1989 and 1990, the University of the Algarve organized two colloquia, entitled 'Archaeology Today', which attracted to Portugal several international figures of archaeological theory.

In the 1980s it was university people who produced the most significant work in Portuguese archaeology. Some of these worked from a solid 'archaeographic' basis and adopted an inward-looking perspective as their approach to, and explanation of, the evolution of societies (e.g., S.O.Jorge on the neolithic and chalcolithic settlements of west Trás-os-Montes (1986); Manuela Martins on the Bronze Age-Roman hill-forts of the central section of the river Cávado (1987), while others chose particularly striking phenomena so as to characterize a certain cultural identity within a culture-historical framework (e.g., Armando C.F.da Silva on the so-called 'hill-fort' culture of northwest Portugal (1986)).

The 1980s not only witnessed the development of long-term regional projects of a kind new to Portugal, but they also served to establish standards which are now used in considering all research applications, whether from within Portugal or from foreign archaeologists. In other words, properly formulated research designs and rationales are now a prerequisite for archaeological endeavour within Portugal. Currently, a large part of Portugal is, for the first time, witnessing archaeological survey activity—an activity facilitated, or even made necessary, by the recent creation of a transport infrastructure.

All this has undoubtedly got something to do with the growing involvement of the state in archaeology. In 1980 the Regional Services of Archaeology were created, with offices in Braga (later in Oporto), in Coimbra and Évora. If this action turns out to have foreshadowed a move towards the creation of more autonomous, less bureaucratic and more flexible bodies concerned with archaeological matters it will have been a significant step forward. Meanwhile, however, the signs are not particularly auspicious, since a former, and very valuable, committee with archaeological and heritage interests has been

disbanded by a 1990 government Act which reflected a generally anti-cultural government stance.

Nevertheless, the hope for the future is that there should be decentralization of power regarding heritage archaeology, especially at a moment when, thanks to funds from the European Union, some large-scale public projects can be undertaken regarding the continuing ploughing up of land for agriculture (an activity which daily destroys vital evidence of the past). Such new projects are also the main hope of retaining, within archaeology, those committed people who would otherwise be forced out of the archaeological discipline.

There is another positive indication for the future: the amount of foreign information about archaeology available to Portuguese specialists has increased to a significant degree, both through direct reading of English and French (nowadays, any Portuguese with a good secondary education is reasonably at ease with these two languages) and, in some cases, also German, and through more intensive participation in international archaeological meetings. Although the number of translations into Portuguese of important foreign works on archaeology still remains negligible (e.g., Binford's *In Pursuit of the Past* has only very recently been published in Portuguese, and this is the only work in Portuguese by a representative of the 'New Archaeology'), contacts with foreign archaeologists are definitely increasing, as is the number of Portuguese authors publishing abroad (and cf. Funari 1995).

Given the above history of what, perhaps, may be thought of as a somewhat marginalized archaeological tradition, it is instructive to note what is happening in Portugal with regard to 'archaeological theory'. Most young Portuguese scholars have tended to adopt eclectic positions concerning theoretical and methodological options. Such eclecticism, or the desire to 'bring together' perspectives which are different or even inconsistent at their point of origin, may well turn out to be advantageous in the fringe context within which Portugal finds itself. Thus, although some people seem to empathize more with a Marxian archaeological point of view, and others more with a 'New Archaeology' viewpoint, and still others with a post-processual stance, the truth is that there is no one favoured Portuguese theoretical approach to archaeological interpretation. Indeed, only recently, Alarcão (1990)—author of several valuable syntheses of a historiographic character about Roman Portugal (and the 'Grand Old Man' of Portuguese archaeology—although he is still only in his fifties)—has compiled a collective work, whose eclecticism can be discerned in every chapter. Meanwhile, in his student seminars on archaeology, he appears to be adopting a remarkable 'recycling' of his perspectives, in that he is now bringing together Marxist and processual points of view (J.de Alarcão, pers. comm), in a, so far, unique Portuguese effort to systematize Portuguese archaeological thought of recent decades. As a result of these discussions we are all the more convinced that many of us wish to adopt positions which are open to various 'schools' of thought. We believe that it is a sign of the growing maturity of the discipine of archaeology that 'schools' should be allowed to coexist and to

continue their dialogues; we also think that it is important to acknowledge different points of view, and different methodologies, which are always influenced by the varying scales of activity which are adopted for the analysis of any particular archaeological example.

In general the Portuguese archaeologist must consider any dichotomy between theory and practice as deeply harmful. On the other hand, archaeology clearly deals with access to the past, over which no one has a monopoly. As a social, and therefore political, construction the past must be retained with open access to it. In resisting any all-embracing tendency to monopolize the past (a tendency which can easily slide into the totalitarian), archaeology clearly does not stand for some kind of 'pure' relativism in which any theory is as good as any other (and see Thomas 1995). Theories themselves have a history, a genealogy, and certain perspectives could not have arisen if others before them had not been produced and, in their turn, had not been put to the test.

In other words, for the enlightened Portuguese archaeologist of the 1990s, there is no such thing as archaeological raw data; all data are produced within a particular theoretical framework, and the assumptions of such a framework must be made available to others so that they may be assessed and controlled by others. Data and theories must constantly interact so as to form, and offer, increasingly richer and more complex 'mental maps' of human experience, not least 'pasts' that can 'account' for more 'likely' ways to conduct our present lives.

The past is, of necessity, subjective. Nor could it be otherwise. The indeterminiation of the/our past is the condition for (our) freedom in the future. Through the 'protocol of proof', i.e., through constant re-evaluation of factual data that are gathered, contrasted, and compared, different perspectives about the past will, in certain respects, create something like a consensus view, a view which will be acceptable to the majority, but which will always remain open to alternative perspectives (and see Thomas 1995, Paddayya 1995, Andah 1995 and others, this volume). Contradictions, incoherence and doubts will never be absent from any construction of the past—such doubts are, in any case, inherent to all pursuits of knowledge—and, equally, they are prerequisites in all enquiries into the mysteries of *the,* or *a,* past.

Meanwhile, when one looks back to nineteenth-century Portuguese attempts to create the past, and appreciates what has been accomplished since then, one cannot fail to recognize the long distances of exploration and understanding already travelled. Nor can one doubt the richness and vastness of data concerning human experience still awaiting discovery. Portuguese archaeologists do not want to remain apart from such discoveries of interpretation and understanding. They should not forget that Portugal has a past, as a discoverer of new worlds, and that Portuguese is nowadays the seventh most spoken language on the planet (and see Funari 1995).

If for no other reason than the above, Portuguese archaeologists need to broaden their horizons, need to have interests of a more global nature. At the

very least, engagement with theoretical archaeology will force them to extend their mental cartography and to depart from the more usual, narrow, 'scientific practice'. Theory and practice do not contradict one another—any good archaeologist must reason clearly at all stages of work, must attempt accuracy at every scale of analysis or synthesis. Those who do not 'theorize' (i.e., those who think that theorizing is a waste of time, a luxury for the idle) and therefore choose to employ 'common sense' theories in their practices, are of necessity people who excavate badly, who fail to publish (or who publish badly) and who present a commonplace and dull synthesis of the past.

NOTES

1 These excavations were initiated in 1930 by V.Correia and expanded in 1955, and the museum inaugurated in 1962.

2 Those in the Geological Services who have been involved with archaeology have had to continue with their non-archaeological activities at the same time.

3 Among the most outstanding representatives of this 'trend' were Sarmento in the Minho (Guimarães), E.Veiga in the Algarve, Rocha in Figueira da Foz, Leite de Vasconcelos in Lisbon (who collected material from all over the country), and a group from the journal *Portugália* (1899–1908) in Oporto (Severo, Peixoto, Cardoso and Fornes).

In 1893 Leite de Vasconcelos founded the Portuguese Ethnological Museum in Lisbon, today's National Archaeological Museum (currently an organization with a completely different philosophy). He left behind him a monumental legacy of writing in the areas of archaeology, ethnography, folklore and philology: particular importance should be attached to his three volumes of *Religiões da Lusitânia* (1897–1913), a remarkable repository of knowledge, currently being researched by several young Portuguese archaeologists.

4 Still including H.Breuil (Palaeolithic), J.Roche (Upper Palaeolithic/Mesolithic) and G. and V.Leisner (megaliths).

5 For example, the (Germans H.Schubart and F.Sangmeister, who devoted themselves to investigating the chalcolithic settlement of Zambujal (Torres Vedras).

6 He moved first to Coimbra and then to Lisbon, where he taught at the University College for Colonial Studies (later the University Institute for Social Sciences and Overseas Politics—this latter designation annulled after 25 April because of its colonial implications).

7 Examples of these include the Work Unit for the area of Sines (from 1972) and the survey and analysis of rock art in the Tagus valley (1971–1973) which, after it had been discovered, was threatened with being submerged under the water of a dam—as eventually happened—so that the largest set of post-glacial rock engravings in the Peninsula was submerged.

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CHAPTER TWELVE
THEORY AND PRACTICE IN IRISH ARCHAEOLOGY

GABRIEL COONEY

INTRODUCTION

On hearing the title of this chapter, one Irish colleague stated that it certainly wouldn't take very long to cover the topic, while another said 'Thank God we have so little of that theory stuff in Irish archaeology.' There has been a marked lack of explicit concern with the theoretical basis of archaeological practice in Ireland. The implicit assumption has been that the information is primary, speaking for itself, that the acquisition of more information is the primary goal of archaeology and that limitations in the data prevent reconstruction of many aspects of life in the past (e.g., Harbison 1988:195). In this empirical tradition, the influence of processual archaeology has been primarily in the area of data analysis and the various strands of post-processual archaeology have been largely lumped together with processual archaeology as 'New', or else have been ignored.

In a wider national context, the profession's internal view of archaeology as an objective presentation of a fractured past is in contrast both with an external perception within Ireland of the discipline as being a highly politicized one (e.g., Butler 1990; Myers 1992) and the discourse within Irish historical research about the meaning and interpretation of the past, and the recognition that history is written in and for the present (Laffan 1991; Dunne 1992). Irish archaeology has, of course, very many positive features and has a high profile within the country. It is viewed as an important activity and resource both officially and by the public. This chapter has a particular focus on Irish prehistory and on archaeology in the Republic of Ireland (consisting of twenty-six counties, three of which are in the province of Ulster), although reference will be made to Northern Ireland (consisting of six counties in the province of Ulster). The question of the relationship between politics, nationalism and archaeology in Ireland inevitably also comes into the debate (see Woodman 1995).

The lack of concern with theory in Irish archaeology may be one of the reasons for the very low profile of Ireland in texts on archaeological theory. Grahame Clark (1957:256–7) commented on what he perceived to be a slow-down in the pace of activity in Irish archaeology after an initial burst of work in

the immediate post-independence period, reflecting as he saw it the political nature of archaeology and its involvement in the establishment of a national identity. In reality the 1930s marked a major period of work within Irish archaeology (see below). Trigger (1984:368) identified Irish archaeology as having the characteristics of a nationalist archaeology and, citing Clark's comments, he posed the question as to why the country had shown relatively little interest in its prehistoric archaeology. He suggested (Trigger 1989:185) that the Viking and medieval urban excavations in Dublin and elsewhere were encouraging a less Celtocentric nationalist approach to Irish history. In Hodder's (1991a) review of archaeological theory in Europe Ireland is not mentioned, but several of the points made in the introductory chapter by Hodder and in the papers by individual contributors commenting on their own countries are very relevant in considering Irish archaeology. First, Irish archaeology has to be seen in its wider European context. The social nature of archaeological enquiry means that Irish archaeology has to be seen as situated within the particular social, economic and political circumstances of this country (Hodder 1991b:19–20). Hodder (1991b: 22) identified the acceptance of the centrality of historical enquiry and the widespread incorporation of Marxist theory as two central tenets of European archaeology. In the case of Irish archaeology it is certainly the case that it has been perceived as a historical discipline (de Paor 1963:112) but, interestingly, there has been very little conscious incorporation of Marxism, despite the high regard that Gordon Childe continues to be held in (e.g., see Herity & Eogan 1977). To some extent at least this can be placed in the context of the general reluctance of Irish academia to engage in social analysis (e.g., Lee 1989:621) and the prevailing conservative religious ethos. While it is certainly the case, as in many other European countries, that the development of Irish archaeology cannot be seen in any way in terms of a progression from culture-history to processualism to post-processualism, it is interesting to ask the question why these broad trends in Anglo-American archaeology have had such a limited impact on Irish archaeology at a time when Irish society has become increasingly tied to the Anglo-American world view (e.g., Kiberd 1984; Lee 1989:666–7).

THE IRISH ARCHAEOLOGICAL PERSPECTIVE ON THEORY

The basic contention set out above, namely that archaeology is socially situated in the present, is something that most Irish archaeologists would seem to regard as irrelevant. While the vast majority of archaeological writing is devoted to detailing the results of surveys and excavations, it is underwritten by an implicit view of archaeology as an objective discipline, a science, explaining a neutral past that can be detached from the present. Emphasis is continually placed on the primacy of archaeological practice. There are pressing national archaeological inventory and management problems (Bradley 1992; Cooney 1992a; Woodman 1992b:295–6) and these have been used to support the notion that an explicit

theoretical perspective can be added in at a later stage when the archaeological resource has been safeguarded for the future. Theory is thus perceived as separate from and not an integral part of practice. As Woodman (1992a:38) has put it, sorting the raw data has been the primary motivating principle guiding Irish archaeological practice.

In this projection of archaeology as an objective discipline there has been very little discussion of the fact that there are implicit theoretical underpinnings to Irish archaeological practice which undermine this image and have a major impact on the way in which archaeological data is gathered and analysed. For example, the richness of the surviving site and monument record continues to dominate policy decisions about the management of the archaeological heritage, despite the recognition of the very partial nature of this surviving record since at least the early 1980s and the evidence of survey and excavation on pipeline routes which indicated the extensive presence of archaeological features and sites surviving only as sub-surface deposits (Cleary, Hurley & Twohig 1987; Gowen 1988). There have been innovations in this field, such as the formation of the Irish Archaeological Wetland Unit with a brief to survey and investigate archaeological features in raised bogs (Moloney 1993), but there continues to be a reluctance to realize or exploit the importance of low-visibility features. One example would be the often assumed attitude that unless there is some known surface indication of a site there is nothing in the ground. This has had a major impact on the way in which, for example, monitoring of motorway projects has been carried out. Second, in the inventory work being carried out by the Archaeological Survey of Ireland and the Archaeological Survey of Northern Ireland, fieldwork has tended to be focused on known sites, with the result that upland and other marginal areas, with potentially the best-preserved relict archaeological landscapes and under the greatest threat from afforestation (Hamlin 1989:179; Moore 1992:226), are in general not as well documented as the lowlands where agricultural and urban activity has been concentrated.

Another frequently expressed tenet of Irish archaeological thought is that because of the limitations inherent in archaeological data there are many aspects of the past that cannot be reconstructed. This is at variance both with the systemic view of culture, as projected in processual archaeology (e.g., Binford 1971), and the stress placed by all the various strands of post-processualism, on the interwoven meaning underlying all material culture (e.g., Hodder 1986:124; Barrett 1994:36). The view amongst Irish archaeologists still seems to be dominated by the idea that culture can be separated into different categories (e.g., Clark 1957:169). There is a reluctance to go beyond the technical and economic aspects of the evidence which are seen as more obvious and straightforward. The concept propounded by Hawkes (1954) and Smith (1955), that there are limitations to the inferences that can be drawn from the archaeological record, and that inference becomes increasingly suspect when discussing topics such as social organization or ritual, is one that still seems to be dominant in Irish archaeology. When such topics are raised, it tends to be in

speculative terms, with little or no discussion of how different interpretations might be evaluated. These views are in striking contrast to current international opinion on the nature of archaeology (e.g., Renfrew & Bahn 1991). In this context it is not too surprising that there has been little discussion in Ireland of the fundamental changes in archaeological theory that have been widely discussed in the Anglo-American and to a lesser extent European literature since at least the early 1970s (e.g., Trigger 1989; Shanks & Tilley 1989; Malina & Vašíček 1990; Hodder 1991a).

Even more striking, however, is the contrast between the view of the past expressed in current archaeological practice in Ireland and that expressed in other disciplines. For example, within historical research there has been an ongoing debate about the extent to which different perspectives on the writing of the history of modern Ireland, for example on the Easter Rising of 1916, reflects the political and intellectual climate of today and the political tendencies of the writer (e.g., Foster 1986; Bradshaw 1989; Ní Dhonnchadha & Dorgan 1991). In particular this debate is concerned with the relationship between historical research and nationalism and a similar concern can be seen in related disciplines such as historical geography (Graham & Proudfoot 1993:13). What this demonstrates is that the interpretation of the past is not unchanging and is very much part of the present.

With the exception of a limited amount of debate on particular issues, however, Irish archaeologists have shown little or no desire to engage in discussion about the influence of politics or nationalism on their work. The importance of the past as a facet of Irish life has been explored in literature; for example by Brian Friel in a number of his plays (e.g. 1981; 1989) and by Seamus Heaney (e.g., 1975) and Eavan Boland (1990) in their poems. These contributions emphasize the complexity and continuing importance of the past for people in Ireland. In all of these examples there is a sense of engagement with explaining the past as a way of understanding the present (e.g., Glassie 1982:602–8) and the importance of material culture as a visible reminder of the past (e.g., Heaney 1985). In their concern with sorting the raw data, Irish archaeologists by and large have not seen this view of the past as a major objective. As a consequence, archaeology as a discipline is still seen as a means simply of providing new information about the past, rather than giving a distinctive perspective based on the interpretation of material culture within a long time-frame. One example of this is in the ongoing debate about the presentation of the past in heritage parks (e.g., Nolan 1991). Archaeologists have tended to be involved primarily in the issue of the validity and details of the reconstruction of buildings or other structures, rather than in the broader topic of how life and society in the past is presented in these parks.

THE DEVELOPMENT OF MODERN IRISH ARCHAEOLOGY

The early 1930s can be seen as marking the establishment of many of the features that are still central to Irish archaeology. In the Republic of Ireland the legislation directly concerned with archaeological sites and objects, the National Monuments Act, was enacted in 1930 (it was subsequently amended in 1954, 1987 and 1994). This provided for the guardianship, preservation and acquisition of monuments, restrictions on the export of objects and the licensing of archaeological excavations (see Herity & Eogan 1977:14). (The legislative framework in Northern Ireland was formerly afforded by the Ancient Monuments (NI) Acts of 1926 and 1937 and is currently provided for by the Historic Monuments (NI) Act of 1971.) The Irish Antiquities Division of the National Museum of Ireland was revitalized under the leadership of Adolf Mahr from Vienna, who was Keeper of Irish Antiquities and Director of the museum from 1927 until the outbreak of World War II (see Kilbride-Jones 1993). Mahr had a major impact on Irish archaeology. He instituted a new recording and archival system in the National Museum where the number of acquisitions greatly increased during the 1930s. He stimulated research in a number of different fields, particularly excavation projects and contributed a major review of Irish prehistory while president of the Prehistoric Society (Mahr 1937). In 1932 the first large-scale scientific excavations in Ireland were conducted by the Harvard Archaeological Expedition and over the next several years Movius and Hencken excavated a range of key sites which were to remain critical for the interpretation of several different periods (Harbison 1988:13). Also in 1932 Oliver Davies and Estyn Evans commenced a campaign of excavation and survey of megalithic tombs in Northern Ireland. Excavation activity was supported in the Republic from 1934 on in the form of government funds to be used to give employment. This appears to have had the direct support of the Taoiseach (Prime Minister), Eamon de Valera. He was later, in the 1950s, to turn the sod to mark the beginning of excavations at the royal site at Tara in Co. Meath (see Woodman 1995) and facilitated the provision of the first radiocarbon dates from an Irish site by ensuring the transit of the samples from Ireland to the United States by diplomatic pouch!

Activity in the 1930s also provided the training-ground for young archaeologists who were to have a pivotal role in the development of the subject. Both Ó Ríordáin and Raftery worked with Mahr in the National Museum in the 1930s and both carried out their doctoral research in German universities. Ó Ríordáin was to go on to be professor of archaeology in University College Cork and subsequently in University College Dublin until his death in 1957. Raftery became the Keeper of Irish Antiquities and later Director of the National Museum. It would appear that an important factor influencing the strength of the empirical tradition in Irish archaeology is these links with German archaeology which continue to the present day. As Härke (1991:198–204; 1995) has

discussed, German archaeology is characterized by a focus on the systematic recording and presentation of material evidence and a reluctance to theorize or synthesize—traits which are also central to present-day Irish archaeology. Another factor which may have been influential in the development of this tradition was a desire in the 1930s deliberately to identify Irish archaeology as practical and non-speculative, with a professional elite whose main role was the scientific recovery of more information about the past. This was supported by the legislative provision requiring the licensing of excavations. This approach would both build on the work of earlier archaeologists in the pre-independence period such as Westropp, Coffey and Armstrong (Herity & Eogan 1977:11–13; Raftery 1988) and serve to insulate archaeology from the criticism of commentators in related disciplines who commented on 'the imaginative and often conflicting speculations of archaeologists and devotees of that curious science that calls itself prehistory' (Binchy 1954:52; see also Ó Ríordáin 1946:162; Evans 1981: 34). Starting with Mahr's review of Irish prehistory (1937) subsequent major statements on Irish prehistory have focused on detailing new discoveries and problems without any fundamental change in approaching the interpretation of the data (e.g., Ó Ríordáin 1946; Raftery 1951; Ó Ríordáin 1955; Herity & Eogan 1977; Harbison 1988; O'Kelly 1989; Woodman 1992b).[1]

Up to the 1970s, about fifty professional archaeologists in university departments (four), museums and monument services formed the core of archaeological activity. Archaeological practice could have been described as primarily focused on research (Woodman 1992a:34–5). Since the late 1970s there has effectively been another major period of development within Irish archaeology, characterized by the growth of the profession in the contract area. There are now in the order of 250 archaeologists working in Ireland. Survey work has focused on the goal of providing a national inventory of sites and monuments and site assessment and excavation work, in both urban and rural contexts, has greatly increased as the results of the surveys have routinely been incorporated into the planning process (e.g., Bennett 1993). There has been innovation in the techniques used in survey and analysis, for example the use of both vertical and oblique aerial photography on a large scale, and the routine use of microcomputers to fulfil a range of functions. But there has been very little change in the supporting archaeological infrastructure or concern with long-term planning strategies (Ryan 1991). The innovations have been largely driven by recognition of the threats to the archaeological resource in the light of the increasing speed and scale of landscape development, in both an urban and rural context. This is not a state of affairs likely to produce by itself a concern with archaeological theory and the well-established paradigm within Irish archaeology, which has its roots back in the 1930s, tends to be reproduced by present-day practitioners.

In 1991 the Discovery Programme, a long-term government-funded research project to enhance the archaeological knowledge of Ireland's past, was established by the then Taoiseach of the Republic of Ireland, Charles J.Haughey.

The Late Bronze Age and Iron Age were chosen as the first focus for the programme and a broad research strategy has been outlined (Anon 1992). Four major research projects are utilizing a range of approaches to examine different aspects of this critical period and the secure, long-term basis of this research suggests that it may well have a critical role in changing research perspectives in Irish archaeology (see Anon 1993a).

THE REACTION TO INTERNATIONAL TRENDS

Irish archaeology is still, however, carried out largely without reference to the debate about the nature of archaeological theory and practice which is going on internationally. There are a number of interrelated reasons for this.

It cannot be claimed that this attitude is a result of lack of information, as the debate has been conducted in journals and books that are readily available in Ireland. It can be viewed as being on the one hand an insular conservative reaction to international trends, in Trigger's terms, the reaction of a nationalist archaeology to an (Anglo-American) imperialist one or it can be seen as a particular expression of a sceptical reaction shared by archaeologists in different European countries about the relevance of theoretical archaeology (Veit 1992: 554–5). But the most basic reason is that archaeologists in Ireland feel that their practice of the discipline is successful, constantly coming up with new information. Ireland has a rich archaeological record capable of throwing new light on all prehistoric and historic periods. As Neustupny (1991:262) has noted, there is a clear inverse relationship between the development of theory and the wealth of archaeological data; the more data, the less concern there is with theory. The number of archaeologists writing in Ireland on broad thematic or period-based topics is quite small and they have a great influence on the more specialized and technical texts which constitute the bulk of Irish archaeological literature. In this type of situation, as Mulkay (1975:515) put it:

> Whereas radical departures from a well defined framework are unlikely to be granted recognition early under normal circumstances, original contributions which conform to established preconceptions will be quickly rewarded.

In the framework of Irish archaeology the main advantage of 'new', or processual, archaeology was seen as its more rigourous approach to data and its stress on the scientific nature of archaeological research, which both added to the image of the discipline and provided the impetus for the development of new areas of research, such as environmental archaeology. On the other hand fundamental tenets of processual archaeology, such as the systemic nature of culture, have had little or no impact on Irish archaeology. One of the few texts which explicitly applies a processual approach in Irish archaeology has been by a scholar based in Britain (Mytum 1992). Irish archaeological reaction to post-processual

archaeology could be defined as minimalist. The emphasis on 'historical particularism' and the importance of context are aspects that archaeologists operating in a traditional empirical mode would see as supporting their view of the flaws inherent in New Archaeology. However, concepts such as relativism and the social construction of archaeological enquiry hold little attraction for a paradigm based on the principle of better answers coming with more data. Again there have been examples of work in which a post-processual perspective (e.g., Thomas 1990; Thomas 1992; Cooney 1992b) has been applied to Irish data but, as in the case of processual approaches, these are still very much the exception.

In two recent papers, Woodman (1992a; 1992b) has addressed many of the issues discussed here. He identifies the fundamental concern in Irish archaeology with sorting the raw data, the lack of re-evaluation of data, and the dangers inherent in this approach for the future of Irish archaeology as a discipline, rather than just a provider of new information. Woodman's (1992b) review of Irish prehistory has been presented as a re-evaluation of the traditional paradigms on which Irish prehistory is based. But as I have argued elsewhere (Cooney 1993) Woodman's paper, while important in demonstrating that prehistoric settlement was more extensive in space and time than had previously been recognized, expresses and reinforces many of the customary concepts used in the study of Irish prehistory. For example, there is an emphasis on environmental adaptation, or stress, as explanations for continuity or change in the archaeological record.

MIGRATION, CULTURE CHANGE AND IRISH ARCHAEOLOGY

In a nationalistic archaeology tradition, as defined by Trigger (1984:360), attention is drawn to the political and cultural achievements of indigenous ancient civilizations as a way of bolstering the pride and morale of nations or ethnic groups. One might anticipate also an emphasis on continuity of settlement and society to emphasize the links between the past and the present day, and as a justification of political independence (e.g., Whittle 1990:216). In contrast, in a colonialist archaeology, one might expect an emphasis on diffusion and migration as a way of explaining cultural change and innovation (Trigger 1984: 361). It is ironic then that migration of new people from abroad continues to be one of the most over-played and under-evaluated themes in Irish prehistory (e.g., Eogan 1991; Stout 1991; Warner 1993). Waddell, in a number of papers (e.g., 1978; 1991/2), has argued for a more complex view of the dynamics of cultural change, citing the evidence for continuity and long-term contacts between Ireland and western Britain and adjacent areas of western Europe. There also have been other contributions that have questioned the evidence for specific migrations, for example in looking at the problem of the transition from the Bronze Age to the Iron Age (Champion 1989; Raftery 1989; Cooney & Grogan 1991). This latter issue is a critical one, as it coincides with the generally accepted date for the Celticization of the country.

In trying to explain the continuing use of migration and invasion as a particular characteristic of Irish archaeology a number of reasons may be suggested. First, Irish history is punctuated by the evidence for migrations—the Vikings, the Anglo-Normans, the Planters—introducing new elites. It does appear that there has been some element of projecting the same process back into the prehistoric period when there are indications of changes in the archaeological record (e.g., see Smyth 1993:404–5). Second, in the 1950s and 1960s, when most of the prominent practitioners in Irish archaeology received their training, it was standard practice to explain change with reference to overseas influence (see Clark 1966). The belief in migration as a major agent of culture change, allied to an empirical perspective which does not readily admit to the possibility of there being equally valid but contrasting interpretations of alterations in the material culture record, has lasted down to the present. In effect, to paraphrase the title of Anthony's (1990) assessment of the role of migration, in Irish archaeology neither the bath, bathwater nor the baby have been thrown out as migration has continued to be employed as a major explanatory device. It is paradoxical that the version of the prehistoric past proffered in schools up until the 1970s was based on the Book of Invasions, an early medieval document compiled c. AD 1100 to provide a mythological version of the past in line with the late Roman intellectual world and the Bible (Champion 1982). This has been replaced in school texts by an archaeologically based version of the prehistoric past which is still stressing the concept of invasion (e.g., Cowman 1989).

ARCHAEOLOGY AND THE POLITICS OF THE PAST AND PRESENT

The issue of the invasion/migration hypothesis in Irish archaeology can be related to the broader question of the nature and degree of contact with Britain versus Continental Europe as evidenced in the past, a duality that underlies questions of Irish identity today (e.g., Graham & Proudfoot 1993:7–8). One could characterize the debate in Ireland as centring on the issue of whether two brands of nationalism—Gaelic and Unionist—will continue as separate strands, or whether a new nationalism can evolve based on the reconcilation of what are historically speaking overlapping and dynamic traditions. As Mallory & McNeill (1991:325) conclude, the identity of Ulster, which lies at the heart of the current national question, has been shown by archaeology and history always to have been a very moveable feast in the past.

Archaeology in practice crosses the modern political boundary between Northern Ireland and the Republic of Ireland, with all-Ireland professional bodies and cooperation. The legislative bases for the protection of the archaeological heritage in Northern Ireland and the Republic share important features, such as the obligation to report finds and the licensing of archaeological excavations, which differ from those in Britain. There has been a certain reluctance on both sides of the border to engage in debate about the nature of links between

archaeology and politics; perhaps because of the lack of explicit theorizing in Irish archaeology, and a sensitivity to the present political climate. It may be relevant also that as a field-based discipline, archaeologists have to deal with the political situation on the ground and the physical expression of the presence of, and conflict between, the two nationalisms in the past. Such discussion as there has been has tended to be anecdotal rather than analytical. Thus, for example, Evans's suggestion (1981:111–2) that de Valera's (1960) proposal of an origin for court tombs (a type of megalithic tomb) on the north-western coast of Ireland (with an ultimate background in France) may have been influenced by a desire to play down links with Britain. Avery's (1990:2) recent comments on the Britannophobia or Britannocentricity of different scholars in relation to the discussion of Celtic art and artefacts in Ireland can be seen in a similar vein. It would be wrong to deny that the historical relationship between Britain and Ireland as colonizer and colonized has a continuing impact on archaeological practice, both in Britain and Ireland. For example, it is rare to see the use of the term 'the British Isles' in a text by an Irish archaeologist whereas it is quite commonly applied by British archaeologists, very often in instances where there is very limited coverage of Irish material. There is a perception in Ireland that the use of such a phrase signifies an underlying and false assumption, that the two islands can be conceived of as always having been a unit, as they were at one stage in the historical period, and as they still partially are. While accepting that Ireland and Britain have a shared past, it is also very clear that the pattern of settlement and society was not only quite different from that on Continental Europe, but also was quite different between and within the two islands.

There are two much more serious issues concerning the relationship between archaeology and politics which need to be critically appraised. First, there is the limited success of the archaeological profession to demonstrate to a wider audience, both those in related disciplines with a special interest in the past and the general public, the complexity and diversity of the archaeological record. Instead, the concentration has been on the production of a cultural-historical narrative. Second, there has been very little in the way of analysis of the connections between the political process and the practice of archaeology (as one of the few examples, see Heffernan 1988 on the background to the Wood Quay saga in Dublin). There is a danger of political objectives leading the way in terms of archaeological policy and expenditure; for example, it is clear that the archaeological heritage is a vital element of the presentation of Ireland as a tourist destination. The government of the Republic is aiming at a 50 per cent increase in foreign tourism revenue by the end of the 1990s. In this situation there is a real danger that spending on archaeology will become increasingly focused on the potential of sites, museums and interpretative centres to attract tourists (see Anon 1993b:63–5).

Irish archaeology needs to be more critically self-aware, more explicit in recognizing the theoretical basis of its practice but also more self-confident in its ability to interpet the past. These changes are necessary if archaeologists wish to

move centre-stage in the debate about the past, rather than having their data used by other disciplines, and out-of-date interpretations simplified and presented as representing current archaeological opinion. By its nature, Irish archaeology will continue to be nationalist in outlook. Hopefully, the national image it will project will be one based explicitly, rather than implicitly as is the situation at the moment, on a reflection of the complexities of past settlement in Ireland. The danger of allowing the past, by default, to be exploited as the basis for ethnically based nationalisms is to been seen in recent political developments in Europe.

NOTE

1 It is interesting to note that R.A.S.Macalister, professor of archaeology in University College Dublin from 1909–43, had provided (1921) a different framework for Irish prehistory, with a thematic as well as a chronological approach, including discussion of social organization, religion and disposal of the dead.

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CHAPTER THIRTEEN
WHO POSSESSES TARA?
Politics in archaeology in Ireland

PETER C.WOODMAN

The world laid low, and the wind blew—like a dust—
Alexander, Caesar and all their followers.
Tara in grass; and look how it stands with Troy.
And even the English—maybe they might die!

(Donncha Dall O'Laoghaire)[1]

The fact that there are two chapters from Ireland in this volume epitomizes Ireland's particular relationship with the rest of the world. It is, on the one hand, a relationship with Europe and the desire to be seen as a modern European nation (see Cooney 1995) and, on the other, an island struggling to establish a particular relationship with Britain, attempting to assess how this relationship has influenced our interpretations of the past and, finally, how much our lives should be seen as a product of indigenous culture rather than outside influence.

It would be too simple to claim that Ireland's view of itself is a product of a colonial past similar to that found in many other parts of the world. At times Ireland has had its own parliament and after 1800 had representation at the Westminster Parliament in London. Yet throughout recent centuries the economic relationship between Ireland and Britain was seen by the Irish as unequal and the will of much of the population of Ireland was frequently ignored. This battle of wills was seen to to be exacerbated by the frequent introduction of groups of people into Ireland with the hope that they would develop Ireland's potential in a manner more convenient for Britain and perhaps prove, as presumed loyal subjects of the crown, more amenable to government from London.

In this context Ireland's past was, and is, just as much a source of political problems as any other aspect of life. In fact, Hoppen noted:

> Since at least the seventeenth century almost every group with an axe to grind has thought it imperative to control the past in order to provide support for contemporary arguments and ideologies…. By the beginning of the present century both nationalists and unionists had each constructed a

> self-contained theatre of the past in which to play out current aspirations against backdrops painted to represent the triumphs of former times… though popular political attitudes have often been expressed in a manner which seems to be "historical", this is so only in the superficial sense that references to past events are involved. For many people it is, indeed, the approach adopted by the writers of the early nineteenth century which holds sway and by virtue of doing so for almost two hundred years, gives continued energy to sectarian attitudes of unyielding resonance and power.
>
> (Hoppen 1989:1–3)

This is not confined to the interpretation of recent historical events. Ledwich in the introduction to the 1804 (post-1798 rebellion) edition of his book on antiquities makes the following observation:

> When Hibernians compare their present with their former condition; their just and equal laws with those that were uncertain and capricious; the happy security of peace with the miseries of barbarous manners, their hearts must overflow with gratitude to the author of such blessings: Nor will they deny their obligations to the fostering care of Britain, the happy instrument for conferring them.
>
> (Ledwich 1804: Introduction)

In one sense this is not new to Ireland's politics, as some of our early Irish history is deliberately created pseudo-history, used by secular society and the Church in early medieval Ireland, to validate (post hoc) any economic advantage or territory that various vested interest groups had come to hold. At the same time, in a modern context, the desire of certain historians to revise our traditional view of Ireland's recent past has sparked off a lively debate in which political epithets are as common as philosophical or critical analyses (see Dunne 1992).

It is in this context that the way that these particular relationships influenced the development of archaeology in the island of Ireland, and the way that archaeology was used to influence the struggle for the repeal of the Union, should be examined. In the case of Ireland there is, of course, an additional dimension, in that archaeology is now administered by two separate governments. It is, therefore, possible to see how archaeology has developed in each region.

The archaeology of Ireland is a very obvious presence on the landscape, so much so that archaeological monuments figure in literature and folk-lore, but much of this is tied into an image of Ireland created sometime in the early medieval period and through texts such as the *Book of Conquests (Leabhar Gabhaile)* which claimed, as most universal histories do, to account for all societies back to the beginning of time. Unlike many other parts of the world where society or a ruling elite claims a relationship with past societies, from 'times primeval' the traditional explanation of the origins of the Irish was the

assumption that Ireland's Celtic population replaced earlier peoples at a relatively recent time in the past, namely the arrival of the Milesians from Spain. Therefore the society which was created in the early Christian (early medieval) period owed much of its origins to a relatively recent influx of Celtic peoples. At the same time it was apparent that many classes of monument found in Ireland could not be explained as medieval or associated with the early Christian period; by the eighteenth to early nineteenth century, academic debate about the origins of the Irish and the builders of monuments abounded. While some, such as General Vallency (1770–1804), were concerned with tracing the origins of the Irish to exotic roots, such as the Phoenicians, others, such as Ledwich (1804), looked to Scandinavia for the origins of certain classes of monument. Ledwich was, for example, convinced that, due to the cruciform shape of the passage and chambers at Newgrange, the monument could only have been built by Danes after their conversion to Christianity, i.e., sometime in the nineteenth century AD. It could be argued that an indigenous people, after a century of revolt and another of systematic repression, followed by severe economic problems, hardly looked like the builders of Newgrange and other monuments. A denial of the importance of earlier indigenous societies would seem a typical attitude of any 'colonial process', while, in turn, the native population will of course seek to identify its Golden Age, which it will strive to recreate. In Ireland this was the early Christian period which was associated with the idea of Ireland as the saviour and protector of western learning. On the civil side this was associated with the idea that the Hill of Tara was an almost fabulous capital of early Ireland. Its importance can be demonstrated by the fact that the brooch found at Bettystown on the Irish coast was renamed the Tara Brooch (Whitfield 1974).

This desire to identify a Golden Age, which helped reinforce a distinctive ethnic character, was one of two competing views of the Home Rule movement of nineteenth-century Ireland. The fight for the identity of small nations of the 1840s spread to Ireland with the Young Irelanders who, in contrast to the parliamentarian Daniel O'Connell, espoused a more radical and eventually revolutionary approach to the repeal of the Act of Union. They had included such literary figures as Thomas Davis, who edited the journal *The Nation*. While obviously claiming an interest in Ireland's past the build-up of Home Rule activity would seem to have gone on through the 1840s side by side with the debate on the publication of the archaeological section of the Ordnance Survey. Only one parish, that of Templemore, was published and the Treasury eventually quashed the idea of a definitive series of publications of the Antiquities of Ireland. While various organizations such as the Royal Irish Academy objected to the lack of publication (Mitchell 1985), it was not regarded as another unforgivable sin which should become a political *cause célèbre*. In contrast, O'Connell's idea of nationhood was based on the needs of Ireland to be seen as a European nation of equal standing to Britain. O'Connell advocated the use of the English language, as the Irish language was thought to be a barrier to commerce, with the result that the demise of the Irish language accelerated through the

remainder of the nineteenth century. Yet even O'Connell was aware of the power of the past and in agitating for the repeal of the Act of Union he used locations such as Tara for large protest meetings. He was eventually faced down by Westminster when his planned meeting at Clontarf was cancelled.[2]

Less political attempts to rediscover Ireland's former glories did, of course, continue. George Petrie visited Tara and with reference to Dinsenneachas, Ireland's first archaeological survey from 1,000 years ago, names were placed on the monument (Petrie 1837). The foundation of the Kilkenny Archaeological Association in 1847 showed, through its rapid transformation into a national society, that there was an interest in archaeology and antiquarianism. This interest can also be seen in the major exhibitions of antiquities in Belfast (1852) and Dublin (1853). The establishment of the first series of the *Ulster Journal of Archaeology* (1853) is another good indication. Yet in the popular mind more exciting explanations were still being sought. While John Windele of Cork and George Petrie debated the purpose of Ireland's monastic Round Towers from as early as the 1830s, even as late as 1866 outlandish claims for Phoenician origins were put forward at an open-air lecture at Clondalkin, Co. Dublin (Mitchell 1985).

It is notable that certain earlier civilizations are seen as the right sort of ancestors. Who wants to be related to the Moabites or the Assyrians? Phoenician origins, on the other hand, have a certain cachet. The Maronite Christians of Lebanon also look to the Phoenicians for their origins (Seeden 1990).

It is only in the latter part of the century that a significant cultural input was added to the Nationalist movement. Concern over tenant rights had galvanized the Home Rule movement in the 1870s, but the foundation of the Gaelic Athletic Association with its interest in sports, in 1884, and the Gaelic League with its concern for the survival of the Irish language, in 1893, gave a cultural and social underpinning to the striving for Home Rule. Initially separate from this cultural movement was a moderate political movement called Sinn Fein, which quoted the parallel of Hungary in demanding autonomy under one crown—Austro-Hungary. Sinn Fein (Ourselves Alone) or abstentionism from the Westminster Parliament, was the way to achieve it.

With these developments went the usual avowed desire to discover Ireland's past. Therefore by 1900, one might expect that there would be a significant attempt to explore Ireland's archaeological past, with the open intent of finding ways of legitimizing the claim to a culturally separate, but equal, nation under the Crown. Tara re-emerged as a source for legitimizing the past, but in a rather different way from what might be expected. The British Israelites came to Tara to excavate the Rath of the Synods, searching for the Ark of the Covenant (Mitchell 1985). In the long run this was an act which could be seen as legitimizing Britain's claim to Ireland.

In fact the 1890s were a period when, along with other aspects of the Gaelic Revival, numerous historical and archaeological societies were set up: the Cork Historical and Archaeological Society, Waterford and South-East Ireland Society or the Ulster Archaeological Society are good examples. In spite of the diversity

of membership, from political activists, such as Francis Joseph Bigger in Belfast or Denny Lane in Cork, to various establishment figures, harmony was maintained. Foster (1992), writing of the Cork Historical and Archaeological Society during its centenary, commends these societies for their balanced, non-political, non-sectarian character. The archaeological and antiquarian establishment would seem to have remained separate from the Gaelic Revival. It is noticeable that key figures such as Hyde, founder of the Gaelic League, Griffiths, of Sinn Fein, Pearse or Yeats for example, were missing from the membership of the premier society, the Royal Society of Antiquaries of Ireland.

Instead there would seem to have emerged by 1900 a popular image of an earlier Irish culture which was to be re-created, an ethnically pure culture of Celtic origins, validated by the introduction of Christianity and reaching its high point in the Early Christian Period—so powerful an image that even the Iron Age myths of earlier prehistoric landscapes could be accommodated in Fairy Forts and druid stones. It is symptomatic of this desire to create a particular historical image that Pearse, when shown a pair of trews (e.g., Fig. 13.1) from the collection of the Royal Irish Academy, was moved to write to Seamus O'Ceallaigh on 26 October 1900 (O'Buchalla 1980:23):

> There is absolutely no means of judging of the age of the garment. Probably it is not older than the 16th century. It must have been worn by some farmer or labouring man and was probably made by his wife. At least I cannot imagine an Irish gentlemen of three or four centuries ago wearing so clumsy an article of dress. Frankly, I should much prefer to see you arrayed in a kilt, although it may be less authentic than in a pair of these trews. You would if you appeared in the latter, run the risk of leading the spectators to imagine you had forgotten your trousers and had sallied forth in your drawers. This would be fatal to the dignity of the Feis. If you adopt a costume, let it, at all events, have some elements of picturesqueness.

While the Gaelic revival did impinge on the re-use of La Tène art (Rynne 1972), it is surprisingly rare in an overt form, e.g., it is absent on the statue to Parnell in Dublin, while in Cork, on the memorial to those involved in the Nationalist movement from 1798 onwards, it occurs only through Erin leaning on a portion of a High Cross (Fig. 13.2). The image of Ireland's past which was being created was a comfortable image of High Crosses, Round Towers (Figs 3–7) and Wolfhounds. Nowhere is this more clearly to be seen than in the cemeteries, where the High Crosses dominate from the turn of the century onwards. Amongst the few examples of a public use of earlier art forms are the Honan Chapel in Cork (Fig. 13.8) which drew heavily on Cormac's Chapel (O'Kelly 1946) and the Public Library in Cork (Fig. 13.9). In fact it was usually in church architecture that a more accurate representation of early Irish art can be found (Sheehy 1980).



Figure 13.1 Trews from Killery, Co. Sligo. The legs, of a check woollen fabric, were held taut with an instep strap. (Photo: the Hunt Museum)

It is clear that the archaeological establishment held aloof from these developments but it did not mean that individuals were not involved. Coffey had been an ardent Home-Ruler, and Cogan was removed from the staff of the National Museum on account of his Republican sympathies (Mitchell 1985). The Home Rule movement had of course become more radical and militant but the impact of 1916, the Anglo-Irish and civil wars, did not ruffle the official composure of the older societies, so that little trace of this traumatic period can be found in publications such as the *Journal of the Cork Historical and Archaeological Society*. In fact it is interesting to note that after the Anglo-Irish War the Royal Irish Academy was concerned that plans to set up a new National Academy might jeopardize not only its existence but also its collection of manuscripts (McDowell 1985).



Figure 13.2 National Monument in Grand Parade, Cork.

With its reliance on language there was little in the way of architectural traditions which could be used to legitimize an Irish state. In fact Ireland's Zimbabwe-Tara, a fascinating field monument does not, to the untrained eye, live up to the expectations generated in song and story (Fig. 13.10). Instead Round Towers and High-Crosses continued as symbols of a distinctive Irish nationalism.

Therefore Ireland became much surer of what it was not, a British domain. So a particular form of deconstruction took place: overt symbols of the Empire were removed, which left Dublin without equestrian statues, and King William III and Lord Gough were no longer politically acceptable. In Cork the memory of King George's statue which once stood on Grand Parade is only preserved in the street's Irish name *Sraid na Copal Bui* (Yellow Horse Street), while the President's Garden at University College Cork hides a dark secret—a large statue of Queen Victoria. There was a removal of monuments and even people who no longer fitted. Sir Bertram Windle, President of University College Cork and first Professor of Archaeology, who had been impatient with the old establishment of the Cork Historical and Archaeological Society and had helped found the more balanced Iverian Society, which had encouraged the study of the Irish language, found his old-style nationalism no longer acceptable and so left to take up a post in Canada.

While archaeologists were not really involved in the creation of the first set of myths, there was an obvious feeling that the archaeology of the last seven hundred years was a subject which should not require too much attention. In spite of the fact that in the 1920s many of the monuments in the care of the



Figure 13.3 Early medieval Round Tower at Turlough, Co. Mayo. (Photo: T.C.Champion)

Office of Public Works were medieval, there was a marked reluctance to tackle the medieval archaeology of Ireland. In fact during the controversy over the excavations at Woodquay in Dublin, those who were reluctant to embark on the extensive excavations which were needed allegedly suggested that there was little reason to excavate, as so many of the deposits were thirteenth-century, i.e., post Anglo-Norman invasion. By the 1970s the archaeology of the Vikings was acceptable but the archaeology of the Anglo-Normans was still something to hesitate over.

At one level, archaeology in Ireland could be accused of its own regional foibles. These occasional forms of gentle Anglophobia were understandable. It is unwise to tell an Irish archaeologist that Irish Bronze Age gold articles were not made from Irish gold, or suggest that hanging-bowl escutcheons were not made



Figure 13.4 A nineteenth-century Round Tower, Ferrycarrig, Co. Wexford; now inside the Heritage Park with a simulated Norman fortification in the background. (Photo: T.C.Champion)

in Ireland (Bruce-Mitford 1985). This search for something distinctive in Irish archaeology created its own dynamic. Throughout the period up to the 1970s, much of Irish archaeology did not indulge in looking for local origins but sought rather to justify Irish European connections by looking for origins for just about everything in mainland Europe (admittedly preferably anywhere but England). This was a reaffirmation of a form of Irish nationalism which goes back beyond O'Connell to the early Republican movement that had links with revolutionary France. It is only in recent years that the search for internal processes of change has begun. This is in keeping with the changes in theory in European archaeology but totally out of step with the ethos of Irish society, which sought internal answers to its needs for the first forty years of independence!

It has, however, been the marked reluctance to deal with the more recent material culture which brings old prejudices to light, and there is only a gradual acceptance by society that Ireland is home of more than one ethnic group and viewpoint. Archaeologists, on the other hand, have always been aware of this fact.

Macalister (1927) was clear that the society of Iron Age Ireland owed much to the 'aboriginal' population:

> This aboriginal population maintained its ground throughout the ages of Stone and Bronze. That it was reinforced by fresh waves of colonisation is highly probably; a gold yielding country is sure to attract settlers. But these were absorbed in the existing population; our osteological material is as



Figure 13.5 O'Meara's Irish House on Wood Quay, Dublin, decorated by Burnet and Comerford in 1870 with a coronet of Round Towers. (From J.Sheehy 1980. *The Rediscovery of Ireland's Past,* London: Thames & Hudson; photo: Edwin Smith)

> yet insufficient to enable us to isolate or identify alien elements. This aspect of the racial affinities of the inhabitants of pre-Celtic Ireland presents one of the numerous questions that still await investigation.
>
> (Macalister 1927:16)



Figure 13.6 A relief ornament of a High-Cross, Round Tower and Wolfhound in the pediment of a pub in Ringsend, Dublin. (From J.Sheehy 1980. *The Rediscovery of Ireland's Past,* London: Thames & Hudson; photo: George Mott)

Yet even Macalister, who often had a European perspective, looked on the archaeology of later periods as being of marginal interest and lesser significance:

> As we review the products of Medieval Ireland, we see everywhere a sad decline from the achievements of Celtic Ireland. All the skill, all the devotion to labour, these are snuffed out like the flame of a candle. Illumination there is none: metal-work is pitifully feeble: sculpture is stiff, formal and uninspired; in architecture there are occasional interesting and even effective freakishnesses, and two or three good buildings, such as Kilkenny Cathedral; but in comparison with Gothic architecture elsewhere, no really notable work.
>
> (Macalister 1927:356)

While even in Britain the architecture of post-1600 was only gradually accepted as worthy of study by the Royal Commission, there has been a significant time lag in Ireland. The very fact that the Irish still talk of the Georgian architecture of Dublin and houses such as Carton and Powerscourt as 'Ascendancy' indicates the nature of the problem (Fig. 13.11). The state has only recently initiated a pilot survey of post-1700 architecture, while industrial archaeology is perhaps even further behind. As Mitchell (1986) has pointed out, the estates of eighteenth and nineteenth century Ireland can be seen as those of the absentee landlord or the preserve of aspects of ecology that agriculture has destroyed. There is a myth that the Derelict Big House was caused by fire-setting by Irish Republicans but Jones (1988) has pointed out that land reform and an insufficient economic base did much, much more to destroy those estates. In a small country with limited resources this distinctive form of Irish architecture has come low down in the list



Figure 13.7 A relief ornament of Erin on the facade of the Central Hotel at Listowel, Co. Kerry. (From J.Sheehy 1980. *The Rediscovery of Ireland's Past,* London: Thames & Hudson; photo George Mott)

of priorities; it compares unfavourably to the work of the people of St Petersburg/ Leningrad in restoring the tsarist palaces of Pavlovsk, Peterhof, Pushkin and Gatschina, after their destruction in the 900-day siege (1941–4).

For the last hundred years or more, therefore, those interested in archaeology were aware of the fact that the image of Ireland as a 'Celtic nation' was not an accurate reflection of Ireland's prehistory. Yet the desire to recapture some of the

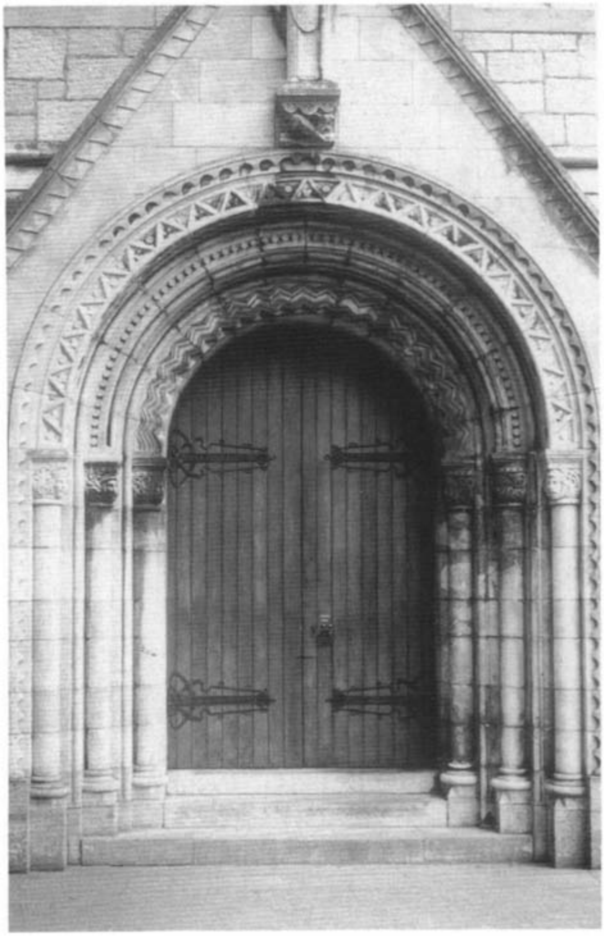


Figure 13.8 Door of the Honan Chapel, University College, Cork, 1915

glory of the island of saints and scholars was so strong that even MacAlister ended his book with the following statement:

> In these tempestuous days of ours, the young Free State of Ireland trims her argosy, and sets forth in courage and aspiration to voyage over the uncharted seas of the future. Four thousand years ago her people guided the first faltering steps of the Folk of the North on the way of civilisation. Twelve hundred years ago they shepherded a war-broken Europe upon the way of learning and the way of Life. May she prove worthy of her ancient past; may she find that once more she has a mission to a bewildered, rudderless world: and may God be her speed in its fulfilment!
>
> (Macalister 1927:357)



Figure 13.9 Cork Public Library (built 1930); one of the few public buildings with Early Insular (so-called Celtic) art

It is only recently that there has been a general questioning of the view of Ireland as a people striving to create a small rural Celtic nation, but it is already too late and it is possible that Irish ethnic exclusiveness has caused problems.

The state of Northern Ireland was in part based on a concept of exclusiveness —namely it did not subscribe to the cultural traditions of the remainder of the island. In one sense this created a vacuum, as Belfast in spite of rich periods of cultural activity was a satellite to Dublin in much the same manner as Cork. The cultural societies and organizations, such as the National Museum, were based in Dublin and the new administration in Belfast was happy to forego any claims over archaeological artefacts such as the Broighter gold hoard found in Co. Derry. These objects were to remain in Dublin while, ‘as Evans noted:

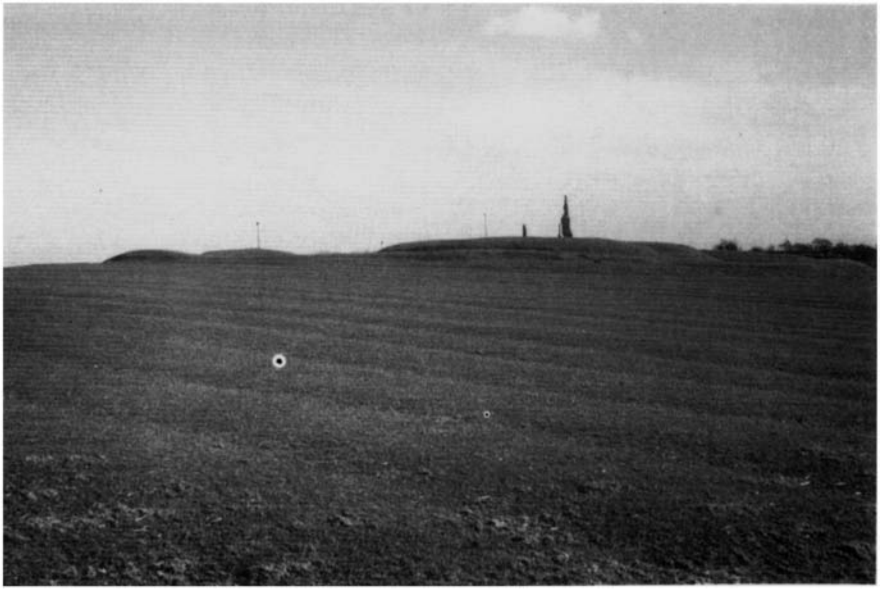


Figure 13.10 Tara, Co. Meath. (Photo: T.C.Champion)

> Monies set aside for cultural centres in the North of Ireland were diverted to the more urgent needs of civil defence; and police stations were built instead of museums and art galleries.
>
> (Evans 1968:4)

In essence, cultural heritage of any type was not an item of consequence for the fledgling state of Northern Ireland, so much so that it was not until the 1960s that the Belfast Museum was finally upgraded to the Ulster Museum.

While this chapter is not intended to be a history of archaeology in Ireland, it is indeed worth noting that, in spite of official indifference, the pre-World-War-II archaeology of Northern Ireland advanced as rapidly as that of the 'Free State'. The Harvard University expedition to Ireland was equally active on both sides of the border and archaeological research was, by the 1930s, being carried out by virtually all the Irish universities. Legislation, which controlled licensed excavation and protected monuments, was also passed by both parliaments and in the North of Ireland a group of amateurs set out in 1934 to produce the *Preliminary Survey of Ancient Monuments of Northern Ireland*.

In spite of the exemplary research record in Northern Ireland, much of the work was divorced from the 'Realpolitik' of the island of Ireland. There was a tendency to begin to write the archaeology of Ulster, or a particular sub-set of the archaeology of Ireland. Even the term Ulster has its own hidden agenda and one can do little better than quote Mallory & McNeill (1991:326):

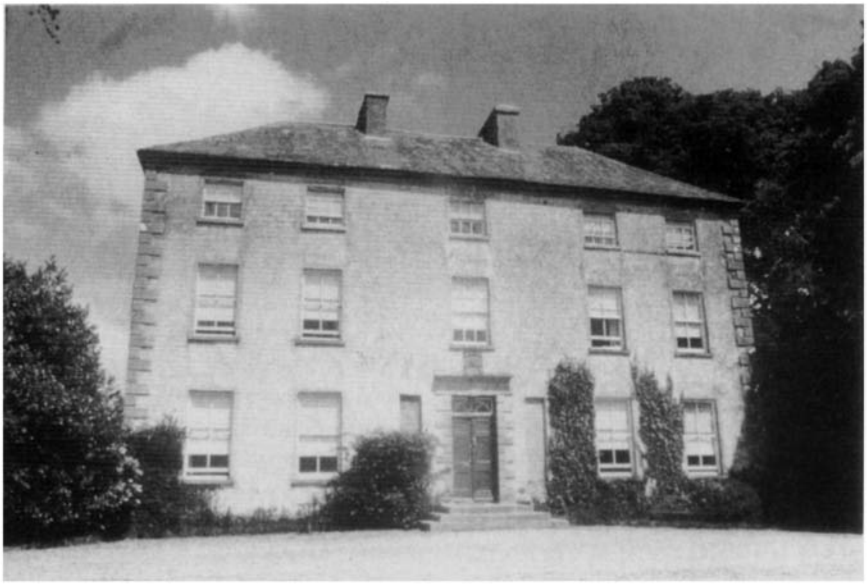


Figure 13.11 Typical 'Ascendancy' house of the eighteenth century; often allowed to decay

> Whatever ethnic or cultural identity lurks behind the name of Ulster is more a subject for study by political scientists or social psychologists than the archaeologists who can readily see that the term Ulster means very different things to different people both "within" and outside the present borders of the province.

Were it not for the tempestuous events of the last twenty-five years, this account would probably end with the observation that, due to the fact that more than 50 per cent of Northern Ireland's post-World War II archaeologists were not born within the province, a more even-handed approach to the recording of all aspects of the archaeological past had taken place. There was no validating mission being undertaken by the Northern Ireland Archaeological Survey, though in retrospect the Co. Down Archaeological Survey reads more like a Royal Commission volume for Southern England than a survey of a part of the island of Ireland. Debate about the significance of certain classes of monument did, of course, take place, e.g., over the origins of court tombs (Evans & Jope 1952; de Valera 1960).

Yet the 'Troubles' have had their own impact. Searching the fine grain of history at local level has led to a vibrant movement organized by the Ulster Local History Federation. For some, however, the search has been far more than local history. The realization that the Westminster Parliament was not an automatic and unquestioning bulwark against change has caused a crisis of

identity among sections of the more militant supporters of the Crown. Loyalty was no longer enough; therefore the concept of Ulster, as always different from and independent of the rest of Ireland, has certain attractions.

Adamson (1974) has argued that certain northern tribes, known at the beginning of history as the Cruithin (Fig. 13.12), were not Celtic but Pictish and that these Cruithin were eventually driven from Ireland to Scotland by the Midland Gaels. Therefore the reintroduction of Scottish planters to Ulster was only a reoccupation of territory lost some thousand years earlier. There are many archaeologists, even within Northern Ireland, who would argue that there is no historical evidence to support this 'interesting scenario', but there is no doubt that certain Loyalist paramilitary groups have found it attractive. Similarly, the existence of the shadowy Tara organization, which may have been created with the active collusion of MI5, argues that the so-called Celts are only recent invaders who have seized lands belonging to peoples that had more in common with their British kith and kin.

It is not the purpose of this chapter to discuss the niceties of Ireland's early history and archaeology but rather to point to several parallels. The myth of a Celtic (and ultimately Catholic) nation excluded a substantial minority one hundred years ago, to the extent that they find it difficult to accept the concept of Irishness and have therefore created their own pseudo-history. In both instances these pseudo-histories, which have rein-forced concepts of ethnicity, have been created at a time of perceived threat. Two hundred years ago many aspects of Irish culture were acceptable to all but, with integration into the culture of the larger island and with the waning of the Irish language in particular a hundred years ago, the need to create a new identity became important. Similarly the new 'Pictish' cultural image of the Northern Loyalists is being created by the threat from outside.

Intellectual independence in any discipline is important, but it is irresponsible to believe that archaeologists have the luxury of removing themselves from society. Their ideas, with all their flaws and political/religious/ethnic bias, are frequently better than the convenient concept the politicians may wish to use. I do not believe that archaeologists have the luxury of saying that it is all relative. They cannot afford the luxury of being supported by others in their ivory tower.

NOTES

1 Quoted in O'Tuama & Kinsella (1981:195).
2 Clontarf was the location of the symbolic defeat of the Vikings in AD 1014.

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I should like to thank John Sheehan for reading a draft of this chapter and in particular for drawing my attention to both the letter of Padraig Pearse and the



Figure 13.12 The Cruithin (Cruthin)—a loyalist alternative to the Celts

poem of O'Laoghaire. While a final version of this chapter was delivered at TAG 1992 in Southampton, it was first discussed in 1990 in Tromso University Museum in Norway. Finally I should like to thank Miss D.Woodman for working with (and typing) the many versions of this chapter.

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CHAPTER FOURTEEN
ARCHAEOLOGICAL THEORY IN JAPAN
HIROSHI TSUDE

> The refinements and maturity of the field are products of a long history during which Japanese archaeologists had very little contact with researchers and theorists working in other areas, and thus developed their own distinctive conceptual and methodological bases…By presenting them with 'archaeological culture shock', Japanese archaeology may help foreign researchers better appreciate the biases and parochialisms of their own particular approach to prehistoric research.
>
> (Bleed 1989:21)

The purpose of this chapter is to describe the historical background of Japanese archaeology in the hope of promoting mutual understanding between Anglo-American and Japanese archaeology.

Archaeological theory in Japan has long been culture-historical in nature. This is clearly evident in that archaeology programmes come under the history department in most universities. Despite the accumulation of a vast quantity of data, sophisticated excavation techniques and the elaboration of a chronology enabling scholars to engage in precise and detailed discussion, Japanese archaeology still suffers from a lack of any epistemological debate. This may often frustrate Western archaeologists (Pearson 1986:4; Anderson 1987:270; Hudson & Kaner 1992:119). This chapter seeks clues to the solution of such shortcomings of Japanese archaeology.

DOMINANCE OF RESCUE ARCHAEOLOGY

The total number of archaeologists in Japan is some 5,700. Archaeologists in universities number 300. There are about twenty-five archaeology departments which are mostly attached to faculties of history. Seven hundred archaeologists work in national, prefectural and municipal museums. Rescue archaeologists constitute the largest group: their number, 4,700, represents 82 per cent of the total number of archaeologists (Tsuboi 1992:3).

The dominance of rescue excavation has characterized Japanese archaeology since the early 1970s (Kobayashi 1986; Tsuboi 1987). Rescue archaeology is well organized. Rapid economic development and the destruction of sites began in the early 1960s. The continuous endeavour on the part of archaeologists to protect threatened sites has received a good deal of support from the general public. As a result, the government has provided funds to set up a good administrative rescue system (Tanaka 1984).

All forty-seven prefectures in Japan have archaeologists specializing in rescue excavations and site-protection administration. In 1990, there were 25,827 excavation activities. There has been a huge increase in the number of excavations since the early 1970s (Fig. 14.1). It should be noted, however, that this number includes many small-scale test-pit surveys accompanying development. Accordingly, the actual number of excavations can be estimated to have been some 8,000. About 3,000 books reporting these excavations were published in the year. The total expenditure on excavations in the year amounted to as much as 84 billion yen (£426 million), coming mostly from developers. Twenty-one per cent of the total was paid by individual and private enterprises, while the other costs were borne by governments and public corporations.

Rescue excavations have provided archaeologists with a vast quantity of data. These well-excavated data not only demand enormously time-consuming post-excavation work, but also prevent the rescue archaeologists from synthesizing the results of their excavations. It is extremely difficult for archaeologists who are swamped by excavation material to develop creative ideas. Such a situation led Saville (1986:43) to comment: ‘Japanese archaeologists are victims of their own success.’

In 1974 the Centre for Archaeological Operations (CAO) was established within the Nara National Cultural Properties Institute: it was intended as an advisory body for methods and excavation techniques carried out nation-wide. CAO has contributed to the development of excavation techniques, the conservation of artefacts and sites and methods of data analysis (Tsuboi 1992:11). These measures have been successful in improving the quality of excavation as well as theoretical sophistication.

The situation of rescue archaeologists can be contrasted with that of university archaeologists who suffer from lack of funding and shortage of staff, which make it virtually impossible to carry out large-scale research projects. Under such difficult conditions, it is particularly those Japanese archaeologists at universities who have published innovative works which fuel theoretical and methodological debates.

The strength of Japanese archaeology is its extensive publication network. There are eight archaeological journals with nationwide circulation as well as innumerable regional ones. Currently, there have been increasing numbers of articles in these journals which deal with theoretical issues. These efforts, however, have not yet been sufficient to improve the overall situation: they seem to be only a drop in the ocean that is flooded with ‘rescue-oriented archaeology’.

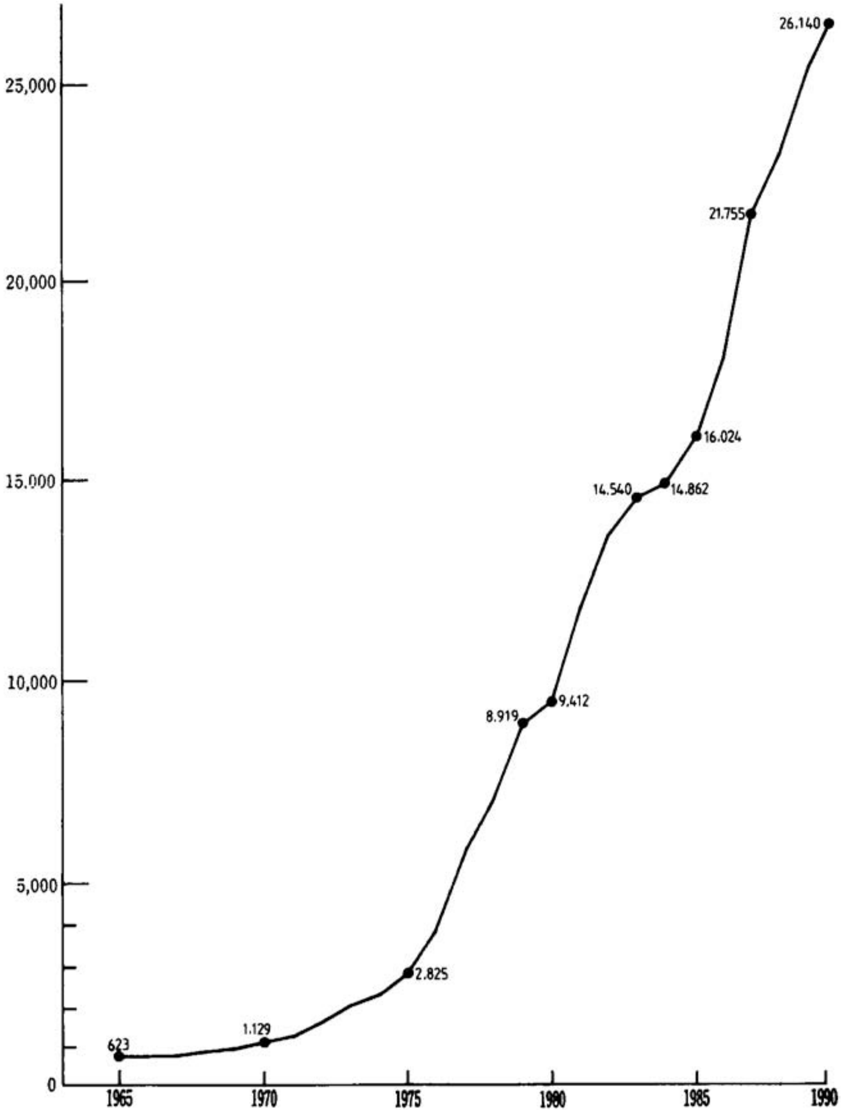


Figure 14.1 Increase in number of archaeological excavations in Japan

These circumstances are closely related to the lack of epistemological debate from which Japanese archaeology suffers.

EVOLUTIONARY THEORY IN THE 1950S

It is the work of Gordon Childe which has been very influential within the theoretical traditions of Japanese archaeology. In particular, his historical-materialist logic, as an explanation of social evolution, has been adopted by many Japanese archaeologists. Six volumes of his works (Childe 1928, 1936, 1942, 1947, 1956a, 1956b) have been translated into Japanese. His diffusionist model of culture change, however, has never been very popular. This is closely connected with the Marxist domination of the study of Japanese history up to the end of the 1960s (see Dolukhanov 1995).

The type of Marxist logic adopted by a majority of Japanese scholars has never been what Friedman (1974) once labelled 'vulgar materialism'. Moreover, some Marxist historians not only sought general laws to explain the developmental stages in Japanese history, but in the early 1970s even developed sophisticated models to explain the role of interaction between regional entities in the formation of the state. Ishimoda (1971), for example, argued that the seventh century was the crucial period for the formation of the state both in Japan and in Korea. He further attached importance to the interaction among polities in China, Korea and Japan for the studies of state formation.

His view was based upon the recognition that chiefdoms in both Korea and Japan had been constantly subject to political influence from China since the third century BC, and that the interaction between Korean and Japanese political groups which had attained to the developed chiefdom stage by the seventh century gave a crucial impetus to the state formation. Although his arguments were related to the more developed political stage than that which Renfrew dealt with, Ishimoda's models appear to have implied similar points to those of 'peer polity interaction' (Renfrew & Cherry 1986), which have been applied by Barnes (1986) to East Asia.

In spite of these pioneering efforts, most historians as well as archaeologists have had a tendency to confuse the concept of 'change' with that of 'development'. This confusion is evident in shifting interpretations of the introduction of wet rice cultivation around the fourth or fifth century BC in Japan. The adoption of this new subsistence system from the Asian continent used to be viewed merely as 'development' from the unproductive hunter-gatherer economy during the preceding Jomon Period (Toma 1951). A recent explanation has challenged this view and proposes that the Jomon people had developed many kinds of sophisticated techniques of food-gathering as well as food-processing, enabling them to achieve a high degree of social complexity (Hayashi 1986; Watanabe 1990). Some archaeologists have even put forward the idea that incipient agriculture already existed (Pearson & Pearson 1978; Kaner 1990:39–40). Thus, the introduction of wet rice cultivation, a drastic change in the mode of life in the beginning of the Yayoi period, has recently been thought to be one of the important elements of a range of techniques which were introduced by some groups of immigrants from the continent.

Currently, more emphasis has been placed on discussions as to the interaction between the immigrants and the indigenous people during the transitional phase (Hudson 1989:60–1; Sahara 1992:43–5). The discussions seem to raise similar points to those made in the explanation of the appearance of the Windmill Hill culture, the ‘Beaker Package’ and hillforts in Britain.

This confusion of ‘change’ with ‘development’, which had been popular in the 1950s, appears to stem from an unquestioned belief, unconsciously shared by many Japanese scholars, in ‘independent development’ in the isolated Japanese archipelago. Generally speaking, most Japanese people have an unquestioned confidence in ‘the unbroken continuity’ of their past. Archaeologists are no exception. This confidence appears to originate, first, in the similarity of modes of life throughout pre-modern Japan, and, second, in the mythology of the pure Japanese.

As to the subsistence economy, rice cultivation and its associated technologies have not undergone fundamental changes since their beginnings in the Yayoi period, except for the introduction of additional improved tools (Kanaseki & Sahara 1976; Aikens & Higuchi 1982; Sahara 1987; Tsude 1989; Sahara 1992). In Japan’s spiritual life Shinto, or the indigenous religion of Japan, is thought to have emerged from animistic worship and agricultural ritual also as far back as the Yayoi period.

MYTH OF THE HOMOGENEITY OF THE JAPANESE

Pre-modern Japan rarely saw large-scale immigration after the establishment of the bureaucratic ancient state in the eighth century. This can easily lead to the belief in the homogeneity of the Japanese. This view particularly encourages overlooking ethnic minorities living in the Japanese archipelago, namely the Ainu and the Okinawans (Fawcett 1986).

The confidence in historical continuity and the belief in the homogeneity of the Japanese people make Japanese archaeologists consider ‘reading the past’ to be an easy task, that is to say, they believe that the customs and ways of thinking of their direct ancestors can be understood without painstaking theoretical work. This is another symptom of the absence of epistemological debate among Japanese archaeologists. Moreover, conservative politicians and business people are prone to want Japanese people to believe that they are members of a homogeneous and harmonious group, naturally unified under the rule of the Emperor (Fawcett & Habu 1990). They appear to be in search of a new form of Japanese nationalism (Fawcett 1990:376).

The current popularity of *Nihon-bunka-ron,* or the pseudo-science of Japanese culture, can only be explained by these circumstances. *Nihon-bunka-ron* consists of a specific kind of argument about the supposed unique characteristics of Japanese culture as compared to any foreign culture. Recently, such arguments have been inclined to emphasize the superiority of Japanese culture as the basis of Japan’s current economic success. These arguments reflect a subtle endeavour,

on the part of conservative groups in Japan, to render legitimate and natural a belief in the superiority of Japanese culture, people and systems, by emphasizing their homogeneity, harmonious nature and historical continuity.

In these circumstances, the mausolea of ancient emperors have great significance (Fig. 14.2). They are huge burial mounds built in the Kofun period (AD 270–600), i.e., archaeological sites of great significance for the study of the formation process of the state in Japan (Tsude 1987, 1992). The imperial household owns all such mausolea, claiming them to be sacred monuments which demonstrate respect for their ancestors. In these circumstances, the mausolea are important political monuments that symbolize the historical continuity of the emperor system in contemporary Japan. Japanese archaeologists have, of course, been requesting the imperial household to give them permission to carry out no more than a general assessment of the current condition of the mausolea. The imperial household, however, has so far rejected requests not only for excavation but even for a general assessment. The *Nihon-bunka-ron,* together with the problems about these mausolea, are typical examples of the ideological manipulation of the past by a specific interest group in modern industrialized Japan.

It is, moreover, noteworthy that Japanese historians, as well as archaeologists, suffered from severe restrictions on the development of theoretical and scientific research, imposed by the ultra-nationalist regime in the 1930s and the early 1940s (Ikawa-Smith 1982; Trigger 1989:179). In this period, it was prohibited by the government even to doubt the divine origin of the imperial household, which was described in the chronicles compiled in the eighth century, namely the official version of history based on the myth of imperial sanctity (Fawcett & Habu 1990:220). The historical descriptions of the imperial household in the ancient chronicles could provide valuable information for the studies of state formation in Japan, if they could be empirically examined by historians. Some historians tried to make critical analyses of the chronicles but these attempts were thwarted by government restrictions. Some of these historians were removed from their academic posts and even imprisoned.

Archaeologists, at that time, turned away from archaeological studies relating to imperial origins, i.e., archaeological research into the formation process of the state based upon mausolea. Instead, they focused upon detailed typological studies of archaeological data, such as the form of mounds and artefacts discovered in the mounds, including bronze mirrors, iron swords and so on: studies which seemed to be less threatening to the ultra-nationalistic ideology (Kondo 1964:312).

Such past experiences appear to have made Japanese archaeologists reluctant to become involved in political issues or even to engage in theoretical debate. These problems epitomize the peculiar conditions with which Japanese archaeologists have to contend.



Figure 14.2 The Nintokuryo mausoleum of the fifth century AD, the largest mausoleum in Japan. The mound has a keyhole-shaped plan, measuring 486m long and 38m high, located in Sakai city, Osaka. It is owned and worshipped by the imperial household. (Photo: Kodansha Publishing Company)

FROM THE EVOLUTIONARY MODEL TO THE STATIC MODEL

As mentioned above, evolutionary models were popular in Japan up to the end of the 1960s. Since then, however, there have been some significant changes.

Figure 14.3 shows changing trends in the subjects of articles published in the journal *Kokogaku Kenkyu (Quarterly of Archaeological Studies)* from 1954 to 1984 (Tsude 1986). The subject-matter of a total of 463 articles has been classified into eleven categories and included in a kind of seriation graph illustrating changes in the number of articles dealing with each topic in consecutive units of five years. This analysis shows that remarkable changes have occurred in the popularity of various subjects and that a structural change began in the early 1970s. A decline can be seen in the following subjects: 'technological development', 'mortuary practice' and 'social stratification/state formation'. At the same time there has been a rise in the popularity of subjects such as 'social structure/kinship relations' and 'trade/interaction'. The decline in 'technology' and 'social stratification' must be closely associated with the increased interest in 'social structure'. This seems to reflect a shift from the

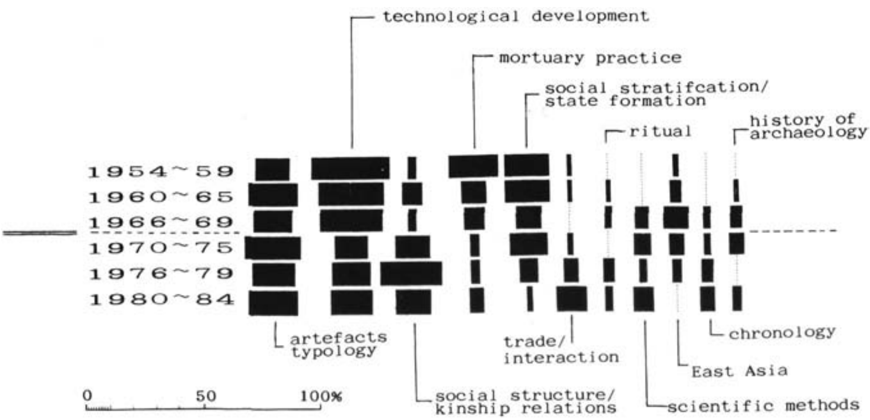


Figure 14.3 Trends in contents of published articles From *Kokogakukenkyu (Quarterly of Archaeology)* 1954–1984

evolutionary model to a static structuralist one. It should be noted that the decline in 'mortuary practices' does not signify the unpopularity of mortuary studies. On the contrary, such studies have recently become increasingly popular, but more emphasis has come to be placed upon the studies of mortuary practices addressing issues connected with kinship relations. This seems to be the background of the rise of 'kinship'.

Some works of Anglo-American archaeologists based on the principles of processual archaeology seem to have had some effect upon these changes. Moreover, work in structuralist anthropology, especially that of Levi-Strauss as well as the *Annales* school, has played a significant role in such shifts in the preoccupations of Japanese archaeology.

It is also noteworthy that archaeologists are children of the contemporary society in which they live their lives and their work reflects trends in contemporary society. The sharp contrast between the decline of the evolutionary model and the rise of the structuralist static model since the early 1970s is very interesting. When Japan enjoyed rapid economic growth in the 1960s 'technological development' was popular among Japanese archaeologists. The early 1970s, on the other hand, saw a number of ecological disasters all over Japan, which were by-products of the rapid

growth of the Japanese economy and, in 1973, the Japanese nationwas affected by the so-called oil crisis. These experiences appear to havecaused a growing scepticism towards the concept of economic and socialdevelopment.

It is also interesting to find that Japanese archaeologists have recently become more aware of the significance of 'trade/interaction', now that contemporary Japan is enjoying a period of economic expansion. In his 'The invasion

hypothesis in British archaeology', Clark (1966) pointed to changes in British archaeology; however, these were in the opposite direction: from an invasion model to an autonomous development model. Clark argued that the shift from the invasion model to the indigenous evolution model reflected social trends in the Britain of the 1960s, where the invasion neurosis seemed to be waning with imperial power itself (Clark 1966:173). It seems that the shift from the evolution model to the interaction model in Japanese archaeology mirrors trends in contemporary Japanese society as a whole.

PROCESSUAL AND CONTEXTUAL

Some scholars have argued that the theoretical developments in Anglo-American archaeology have had little or no effect on Japanese archaeology (Pearson 1986: 4; Hudson & Kaner 1992:119). On the contrary, it is noteworthy that some methods in processual archaeology have been adopted by Japanese archaeologists: studies of demography (Koyama 1978), settlement patterns (Kobayashi 1980; Sakai 1984) and seasonality in subsistence activities (Koike 1980; Akazawa 1981; Suzuki 1986) are good examples. Some archaeologists have even developed ethnoarchaeological studies of food gatherers in the Japanese archipelago (Watanabe 1968; Koyama & Thomas 1981; Anzai & Sato 1993).

In spite of these pioneering efforts, most archaeologists in Japan have adopted some ideas from processual archaeology only in order to elaborate their own archaeological method. They have accepted processual archaeology just as a 'scientific' approach to data analysis. To them, the methods of processual archaeology seem analogous to the three-dimensional recording method which was developed by Leroi-Gourhan (Audouze & Leroi-Gourhan 1982:172–5). Such an attitude in Japanese archaeologists towards processual archaeology seems to be closely related to their 'reliance on empirical research' (Fawcett 1990:374), which has long been a preoccupation of Japanese archaeology. More emphasis has been placed upon the method for producing 'facts' of the past, rather than on elaborating a theoretical framework.

Few Japanese archaeologists have either adopted systems theory or sought to develop a covering law to explain cultural processes. This, indeed, is the reason why they have rarely become aware of the necessity to improve the shortcomings of processual archaeology. Habu (1989:141) has pointed out the absence in Japanese archaeology of the increasing scepticism currently shown by Anglo-American archaeologists about objectivity in archaeological interpretation.

Contextual archaeology seems to have emerged as a sort of digestant for Anglo-American archaeologists who had suffered from stomach-ache after eating too many heavy steaks called processual archaeology. Such medicine may be felt to be unnecessary for most Japanese archaeologists, who have only tried a small tasty portion of the steak, i.e., some ideas of processual archaeology useful for the elaboration of method. In these terms, Japanese archaeology has been

neither processual nor contextual. Japanese archaeologists, however, can gain access to contextual archaeology in a variety of ways:

1 'Minority viewpoints' (Hodder 1984), emphasized in contextual archaeology, would seem to be potentially of particular importance to Japanese archaeologists, who have paid little attention to objectivity in the past. It is important, for example, to re-examine the unquestioned confidence in the homogeneity of the Japanese people and to clarify the past of ethnic minorities. In doing so, it is essential to clarify the contextuality between the past and the present in order to make Japanese archaeology more objective. In this sense, 'to be contextual' does not imply 'to be subjective'.
2 Japanese archaeologists have studied Japanese history in close collaboration with historians. These cooperative studies have made it possible to explain the historical contexts of archaeological records. They have been successful, especially in research into the formation process of the Japanese state, where both historical records and archaeological materials can be combined (Tanaka 1987; Barnes 1988). Moreover, the last decade has witnessed remarkable developments in historical archaeology, especially in the study of cities, rural landscapes and pottery trade in the medieval and early modern ages. These results may help archaeologists to produce a theory to build bridges between the past and the present (Hodder 1991:105).
3 The question of how to obtain the support of the public for archaeology is significant, in both academic and practical terms. It is noteworthy, in this context, that some Marxist historians helped form the 'people's history movement' in the 1950s. Their motto was 'history should be studied for the benefit of people living in the present'. Although this movement was criticized later for having been too subjective and pragmatic, its original idea, 'to study history in terms of the present', still seems valid. The excavation of the Tsukinowa burial mound in the Okayama prefecture, carried out in 1953, was part of this movement. At Tsukinowa, local people participated in the excavation with archaeologists in order to understand Japanese history with the aid of their own hands (Kondo 1960:398–417, cited in Fawcett 1990).

It is these historical circumstances that have enabled archaeologists successfully to carry out campaigns to protect archaeological sites and to receive a great deal of support from the general public. Communication between archaeologists and the public has made it possible to help promote discussions about the significance of the past for farmers, craftsmen, women, ethnic minorities and so on. These discussions have given archaeologists stimulating ideas on how to explain the past in terms of the present. Furthermore, it seems important for Japanese archaeologists to have much more communication with the public to prevent the 'ideological interpretation of the past by groups such as media, government agencies and the tourist industry' (Fawcett 1990:374).

CONCLUSION

In Japan, archaeology as a whole has long been thought to be a study of cultural history. Some scholars applied the theories of Childe or the Marxists to construct an explanation of the historical development of Japanese society. These theories were thought to be useful as types of general law. Few scholars, however, attempted generalization at a global level. This may be partly because of the insularity of Japanese archaeology. Accordingly, it is now essential for Japanese archaeologists to discuss their framework in worldwide perspectives.

Moreover, it is noteworthy that Japanese archaeologists, as well as historians, have learned a lesson from the restriction on the development of theoretical and scientific research, imposed by the ultra-nationalistic regime before 1945. These circumstances lead Japanese archaeologists to attach importance to the idea of studying history in terms of the present. They have developed 'their own distinctive conceptual and methodological bases' (Bleed 1989:21). Such characteristics developed in Japanese archaeology may provide Anglo-American archaeologists with some clues to transcend the debates between 'the processual' and 'the contextual'.

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CHAPTER FIFTEEN
ARCHAEOLOGY AGAINST THE STATE
Roots of internationalism

CHRISTOPHER EVANS

From World Heritage Sites to World Systems, global perspectives have come to play a dominant role in archaeology. The proliferation of variously 'World'-entitled journals and the publication of the many volumes that are the proceedings of the 1986 World Archaeological Congress (One World Archaeology) attest to the prominence of the idea of a world archaeology/prehistory. There is a tendency to place the birth of this highly influential concept some time in the 1960s, the decade of emotive satellite imagery of the planet and McLuhan's 'global village'. Capturing the public spirit, 'World' projects featured in United Nations/Unesco and environmental initiatives of the 1980s. Interrelating the global and local, the very big and small, by transcending or bringing pressure to bear upon the 'middle'—the nation—'One World' is a compelling and apt contemporary ideology. It has come to supercede 'united nations', whatever the basis of their unity, inasmuch as it suggests the possibility of a world without borders.

This chapter explores the roots of this recent archaeological internationalism, postulating that its origins, at least in part, lie in late/post-World-War-II reactions to nationalist archaeologies of the 1930s. Marking a watershed for the discipline, this reorientation was largely determined by the question of *the role of the state* and encouraged by the promise of objective archaeological science. The 'state' issue, much to the fore in the 1930s and 1940s, is still of relevance. It reflects, for example, upon recent declarations of politically motivated archaeologies and the awareness of practice in relation to local/indigenous, national and international concerns.

'The World' and its many pasts is a (too) vast arena. This study is concerned only with the formulation of an international perspective in British archaeology. Narrowing the focus even further, it specifically considers the 'announcement' of this concept within a conference held in London in the late-war years[1] by Grahame Clark and his subsequent fostering of a global archaeological programme in the decades preceding the publication of *World Prehistory* in 1961. Finally, comparison is made between Clark's 'project' and the avowedly political agenda of latter-day (One) World Archaeology.

'ARCHAEOLOGY AT HOME'—A FREE FOR ALL

> The cardinal fact about British archaeology is its decentralization…this position is fundamentally healthy and sound—according as it so evidently does with the democratic spirit alive among English institutions.
>
> (Hawkes 1938:47)

What should be a crucial, but is a seldom cited, document in the history of British archaeology is the *Proceedings of the Conference of the Future of Archaeology* held at the Institute of Archaeology, London, in August 1943. Published in the Institute's Occasional Papers Series, it includes not only the twenty-nine delivered papers, but also submitted correspondence and the transcripts of ensuing discussions (Conference on the Future of Archaeology 1943; hereafter CFA).

Christopher Hawkes apparently declared the meeting a 'free for all' (CFA: 83). Remarkably mixed in its participation, its *Proceedings* are extra-ordinary inasmuch as they provide unguarded insights into the complex interrelationships then existing within British archaeology. It was not the habit of the time to proclaim disagreement within published papers and in this regard the discussion portions of the volume are of the greatest relevance. The field was small and publication outlets few. Debate could occur face-to-face and rarely needed to enter the printed page. As a result there is only limited documentation of conceptual change. In contrast the CFA could be considered 'archaeology as theatre' (cf. Tilley 1989) and the frank exchanges between players belie observations that the discipline of the day operated as some kind of 'gentlemanly golden age' (*pace* Chippindale in Daniel & Chippindale 1989:11).[2]

Typical of congresses at the time, it included state-of-the-art summaries and forecasts for the future of prehistoric, Romano-British and medieval archaeology in Britain. Yet the self-congratulatory tone often common to such gatherings is lacking. Its *Proceedings,* in fact, make for rather salutary reading, for many of its themes are still all too familiar. Kenyon, Beazley, and Harden stressed the need for training in excavation, analytical skills and interpretation (CFA: 39–41; 42–4; 44–6). The issue of professionalism was raised *vis-à-vis* the need for non-academics to earn a living and whether excavators should be licensed (CFA: 50).[3] Fox called for the creation of a National Artefact Card Index (CFA: 51–3) and pleas were made for a series of archaeological guidebooks. The role of national and local museums was discussed, as was the importance of archaeology in universities, elementary, secondary and adult education. The future of overseas schools, expeditionary funds, and local societies were all debated.

This chapter is not, however, a belated conference review fifty years after the fact. In many respects it was not just a conference. International events focused attention so that British archaeology was able to 'see' itself and exposure to extreme foreign nationalisms encouraged 'homeland' introspection.[4] In the *Proceedings* we see the archaeological fraternity on the brink of 'disciplinary

modernism' (Evans 1989:446–7), which many at the conference were at pains to avoid. Occurring as it did late in the war (when victory was in sight), and against the spectre of German National Socialism's use of archaeology (when the full horror of Nazi atrocities was becoming known), the conference was concerned with the interrelationship between archaeological professionals and amateurs; civil servants and academics; and the practice of regional, national and the possibility of an international archaeology.

The question of state sponsorship loomed large and, to some degree, divided the CFA conference. Crucial papers on this theme were delivered by Myres, 'The Need for Planning' (54–6), and Grimes, 'Archaeology and the State at Home' (65–9). The former openly addressed the issue, 'does a planned archaeology imply a state controlled archaeology?'. For fear of threatening the British amateur tradition and independent initiative (wealthy 'Big Men'), Myres argued that the role of the state should be essentially negative, to protect the records of the past. There were grave risks should it assume a positive role and direct research. This he thought should be undertaken through local and national societies under the auspices of the then anticipated Council for British Archaeology.

In the discussion that followed, Myres' stance was heartily endorsed by Clark:

> There is no real scope for argument on the necessity for planning…. I think that all of us have had experience of Government or Service Departments during the last few years, and I feel very doubtful whether this experience has made us more enthusiastic for further contact.
>
> (Clark CFA: 62)

and supported by others:

> It seems to me that what the speakers want by State Control is money, and many wish to have security which very often means stagnation. We need the amateur—the person who will do archaeology for the love of it and is willing to put up with hardships rather than give up the joy of the actual work itself. State aid must mean State Control, which will inevitably reduce us to the level of Civil Servants. You will find very little initiative in the Civil Service. If you want originality in a science do not have State Control.
>
> (Murray CFA: 63)[5]

Having spent the war with the Ministry of Information, Jacquetta Hawkes sharply retorted:

> *Archaeologists seem to be a flock of sheep flying before the big bad wolf of State Aid.* Surely we are being unrealistic. Is the Civil Servant a different species? The private societies have not been unwilling to enter the British Museum in search of advice, and that is the State. We must arrive at some

> proper relation between research and the Treasury. What is the State except ourselves.
>
> (J.Hawkes CFA: 64; emphasis added)

Not surprisingly, Grimes (then employed as an archaeologist by the Office of Works) argued for further government involvement:

> The almost complete absence of general control in the past has produced a shocking mess in our countryside in which antiquities have been by no means the only casualties. These antiquities are the irreplaceable raw materials of archaeology. They can in the end only be protected through the State.
>
> (Grimes CFA: 65–6)

In his paper he went on to advocate, among other points, the need to control fieldwork ('We can no longer afford to allow excavation by the untrained, even in the sacred name of individual freedom'), that a governmental excavation grants board should work in conjunction with independent archaeological bodies, and that state organization was necessary for certain aspects of large-scale research (e.g., national mapping/survey and aerial photography).

The discussion that followed opened with solicited statements on the organization of archaeology in France and Germany. Clark, developing upon a theme of his opening address, responded:

> Many speakers have stressed the National interest, but I would stress rather International interest. In efficiency of method, pre-war Germany was exceedingly far advanced. I myself have admired it for years. *Card indexing, record, and so on are all magnificent. We are years behind, but where has it led Germany, and where would it lead us?*
>
> (Clark CFA: 70; emphasis added)

The debates and divisions that emerged on the floor of the CFA have, until very recently, continued to characterize British archaeological practice. Compared with continental counterparts (and some of the nation's colonial fosterings) it has had a weakly developed and poorly integrated state service with only limited interaction with the academic community. A strong amateur involvement in excavation has hindered the development of professionalism, and individual initiatives have been favoured over largescale coordinated research projects. Moreover, Cambridge, the 'flagship' of archaeological departments in the United Kingdom, has very much seen its mission as lying in the world at large, rather than the nation.

RE-WRITING THE PAST

Apart from a general discontent with wartime civil bureaucracy and the extension of governmental powers, in the 1943 conference opposition to state-organized archaeology essentially stemmed from two factors. On the one hand, many saw any move towards centralization as being anti-individualistic (i.e., 'English'). Imposed professionalism was equally a threat to personally inspired initiatives and amateur group efforts. On the other hand, at least for Clark, it carried the possibility of totalitarian distortion inasmuch as national archaeology was considered inevitably nationalistic.

Yet it is difficult to understand how (paraphrasing Clark above) card-indexing could be envisaged as the root of all evil or, at least, a threshold to state misuse of the subject. This seems too much of an over-reaction. The radicalism of Clark's stance can only be appreciated if these conference debates are considered within an historical context. When, during the later 1930s, leading British archaeologists were trying to gain national acknowledgement and support for their subject, Clark expressed considerable admiration for the organization of German archaeology under the National Socialists. He himself admitted in 1943 (Clark 1943:119, note 5) that, if written then, the views put forward in the concluding chapter of *Archaeology and Society* of 1939 concerning and contrasting Soviet, German and Italian state archaeology would have had different emphasis.[6] Coming from the founding-father of *World Prehistory,* Clark's change of heart towards state sponsorship is crucial. The strength of his internationalist convictions was surely engendered, in part, by his earlier (albeit qualified) support of pre-war archaeology in Germany. Caught out by world events, it was within the 1943 conference that he denounced state practice.

Only a summary of Clark's introductory address, 'The contribution of archaeology to the post-war world', appeared in the CFA *Proceedings;* the full text ('Education and the study of man') was published in *Antiquity*. In it he stressed the vital role that education must play in the future:

> We stand on the threshold of what could be a new world: whether we cross that threshold or are elbowed back into the dark passage that leads to another holocaust, depends primarily on our attitude to education, on the steps taken during the next few years to bring to the common man everywhere a realization of his inheritance as a citizen of the world and an awareness of his power to mould his own destiny. What is needed above all is an overriding sense of human solidarity such as can come only from consciousness of common origins. Divided we fall victims to tribal leaders: united we may yet move to a life of elementary decency.
>
> (Clark 1943:113)[7]

With its secularisation, however, education had become over-specialized, in need of radical reform and the extension of its franchise. Furthermore, Clark argued that the 'solidarity of civilized men' had been undermined through nationalism:

> If in the political sphere it has become obvious that national and even regional allegiances must be abated in favour of a world allegiance, so cramped has science made our human living-space, it must be equally clear that some scheme of education is required, common to the whole human race, understandable by all and tending to a common norm of conduct, being based fundamentally on the biological unity and the cultural inheritance of mankind. In a word education must be re-integrated on the broadest possible basis, nothing less than the universal experience of man.
>
> (Clark 1943:114–15)

Going so far as to present his own syllabus, Clark proposed that anthropology and prehistory had a unique role to play , being a bridge between the arts and sciences (Clark 1943:115–16):

> the realization of a world community depends on the existence of a common human past on which a common tradition may be founded… education everywhere should be grounded and based on the common experience of humanity, its emergence from the world of the beasts, its age-long struggle for betterment…. *Had the German, Italian and Japanese peoples of the present generation received a grounding in the natural and cultural history of mankind, its seems impossible that they could have been mesmerized by the crazy dreams of racial and cultural domination which today are sweeping them to ruin.*
>
> (Clark 1943:118, 119; emphasis added)

This was an ennobling and passionate vision of archaeology.[8] 'World allegiance', 'common/universal experience of humanity/man': the employment of such phrases marks a sea-change in the perception of the role of the discipline in the world at large.

During the 1950s Clark continued to advocate a world prehistory. Though acknowledging the importance of the university's contribution to British archaeology, in his 1954 inaugural Disney lecture it was Cambridge's international scope that was emphasized:

> The veneration of antiquity is surely an emotion worthy of cultivation both for its influence on individuals and as making for the closer cohesion of society. Reverence for a common past is the sheet-anchor of patriotic feeling. Is it altogether too fanciful to suggest that the study of world prehistory may even help to nourish the solidarity of mankind on which our well-being, if not our very existence, depends?

(Clark 1954:35)

Many of these themes were reiterated in his presidential address to the Prehistoric Society five years later. Though it was referred to in the earlier papers, here the fact that technological advance demanded a world perspective was stressed:

> our studies will gain immeasurably if we only see them in the widest perspective. *Personally I would go further and admit that for me prehistory is only worth pursuing because it sets not merely history but contemporary life in the kind of perspective allowed, and in the final resort necessitated by modem science….*
>
> Today men nurtured in their own distinctive and parochial manners, beliefs, art conventions and histories, and situated at the most diverse levels of economic and cultural development find themselves caught up in a world that for a variety of reasons, not least among them the possession of weapons of unparalleled power, cannot long survive without a common sentiment and allegiance more positive that the fear of mutual destruction. *Modern science has created conditions under which autonomous histories are not merely obsolete but even pernicious.*
>
> (Clark 1959:12–13; emphases added; see also Clark 1946:104 concerning the menace of science)

The impetus behind the publication of *World Prehistory* (1961) can be traced through this series of papers. Yet while world prehistory is widely recognized as having been Clark's project, it did have other advocates. He acknowledged Crawford's pioneering efforts as editor of *Antiquity* (Clark 1959:12) and, though lacking Clark's sense of missionary zeal, in his late-war addresses Childe also argued that archaeology must thereafter be international:

> Archaeology is a branch of history, one of the humanistic disciplines. But its concrete, substantial and objective character makes it a social science too, that is to say, an aspect of human activity that is the same for all humanity—a true universal, and therefore peculiarly a basis for amicable international cooperation.
>
> (Childe 1944a:7)[9]

This could, however, only be achieved by avoiding recourse to 'subjective speculative interpretations' generated due to the paucity of 'archaeological facts' and, though not advocating a state practice, he tacitly accepted its existence:

> The areas of archaeological exploration in fact coincide with political divisions, for the study of antiquities is nearly everywhere more or less

> dependent on Government funds, and in any case is always a function of the social and economic development of a political society, i.e. of a State. *Therefore the elimination of metaphysical speculations that are liable to nationalistic or other distortions and the substitution of objective archaeology and legitimate controllable deductions therefrom must depend upon international co-operative effort.*
>
> (Childe 1944a:7; emphasis added)

Reactions to pre-war nationalist archaeologies clearly involved attitudes towards science. More objective practices, focusing upon concrete facts, and international 'science' were to counterbalance national subjectivities ('perversions'). Yet Clark's appreciation of science appears to have been somewhat two-sided. While its archaeological promise and capacity for international exchange was often referred to, it was after all the terror of modern technology that necessitated world solidarity. In the end, nevertheless, it was science that provided the universalizing linchpin for Clark's 'World (as) project'.[10]

On the whole Childe's concept of international archaeology was the more traditional. It represented the extension of established European practices into an overseas arena. Despite caveats concerning modern science, Clark's concept was much more idealistic—a liberal world 'vision'. These differences related, in no small part, to their respective fields of research. Childe, the later prehistorian of 'complex societies' (and a Marxist), was concerned with detailed typologies/ chronologies and the interaction of cultures. Whether it was his status as a colonial outsider that encouraged his Eurocentrism, at heart he was only really interested in Europe and its interrelationship with West Asia (Trigger 1992). Clark's earlier 'Stone Age' interests were more appropriate to broad-brush international approaches (i.e., 'the archaeology of arrivals') and his was a field in which far-flung ethnographic sources had greater applicability. In other words, because it did not stem from a distinctly national tradition, Clark's archaeology was the better suited to go out into the world.

ABROAD 'AND BEYOND'

> Europe is very sick, and the recovery of its health, intellectual and spiritual no less than physical, will only come gradually, by striving and by patience. The question of a remedy for Germany, alone, is obviously one of appalling difficulty; and east, west, north and south in Europe we have all to get to know each other better…while we can safely say that most of us respond readily to the attraction of archaeology in our home countries, we surely can say more: *there can be few of us, I suggest, who would wantonly disown the attraction of archaeology abroad.*
>
> (Hawkes 1944:74; emphasis added)

Although radically redefining its agenda, Clark did not invent international archaeology. There had been international congresses since 1866 (Daniel 1950: 313–14). These, however, were largely European-oriented gatherings. While roughly representative of those countries where archaeology was currently practised, overseas participation was limited.[11] There was little sense of international coordination or cooperative effort and the papers presented were essentially straight-forward accounts of fieldwork and regional researches.

It was colonial enterprise that gave rise to archaeology abroad (see e.g., Tanudirjo 1995; Andah 1995; Paddayya 1995; Politis 1995; Funari 1995). Foreign investigation was an accompaniment to, and the perogrative of, empire. In fact, before World War II international archaeology remained little more than a narrative of European and West Asian development with overseas embellishments: 'the task of the antiquary was evidently to carry back the story of his own society, or at any rate of the traditional civilization in which he shared…other peoples' histories and antiquities were no vital concern of his' (Clark 1959:13; see also Scarre 1990:14). Hand in hand with the establishment of distant British Schools and Exploration Funds, banner headlines of the day in, for example, *The Illustrated London News* sang the praises of the nation's great 'spade men'. Their discovery of early 'high' civilizations, whose study was detached from later developments ('decline') and contemporary inhabitants of the region, effectively lifted them from any local context and created a 'world culture' of civilizations—the archaeology of empires was a pursuit fitting empires.[12]

Nineteenth-century interest in things foreign also related to the influence of comparative anthropology and the concept of progressive cultural evolution upon archaeology—developmental stages in the European past had their corollary in contemporary ethnographic exemplars, whose retarded progression was attributable to geographic factors and racial/ethnic character. This created an internationally referential framework of European prehistory, albeit fundamentally racist. It was a framework which permitted, for example, the juxtaposition of detailed Bronze Age metalwork typologies with the mythologies, dress-/house-styles of diverse aboriginal peoples, national folklore and the world's 'rude stone monuments'.

Arguably the greatest achievement of Clark's 'world project' is that he formulated a truly *global archaeological perspective* divorced from such concepts of progressive cultural development. While such a development was perhaps tacitly recognized previously by others, in his Introduction to the second edition of *World Prehistory* in 1969 (and later papers; e.g., 1979) he firmly laid this notion to rest. What is, of course, relevant in this regard is that this explicit anti-imperialist perspective is not to be found within the first edition of that volume (1961), suggesting that he had not appreciated the underlying significance of the 'project' from its outset.

The many roots of international practice are evidently embedded within the history of the discipline. One must, therefore, be wary of assigning its recent

resurgence to an individual and a late-war conference episode. It would certainly be an oversimplification to just attribute the (re-)birth of a world archaeology to reactions to the nationalistic practices of the prewar years. Other factors contributed: post-war idealism, belief in scientific truth, a modernist sense of a 'plannable' international future (typified by the creation of the United Nations in 1945), and also the knitting together of the globe through modern communications and modes of transport.[13] Nevertheless, for a time world prehistory was Clark's project and it was his wartime re-evaluation of the evils of state archaeology that first directed him towards internationalism. It is this reactionary linkage that the latewar London conferences give insight into: not just born on a tide of 'brave new world' idealism, world archaeology also grew as a response to pre-war practices. This is a factor which the discipline, if not writing it out from its history, has at least all but forgotten. In the United Kingdom the effects of this neglect are acute. Without an appreciation of the major divisions which arose during the mid-1940s in relationship to the issue of state archaeology it is impossible to fully understand its subsequent development, primarily the extent to which its academics abandoned the 'national' in favour of the 'world'.

Post-war world prehistory was fostered in a functionalist programme driven by ecological/economic determinism—'the biological unity of man'. It could, if somewhat cynically, be interpreted as essentially a neocolonialist 'crusade' relating to the decline of empire in the post-war period. Yet while it was an archaeology without national bounds or traditions, its remit has not so much been international (i.e., between nations) as global or *extra*-national. Its persuasion and far-flung networks related to the spread of universities (and the dispersion of Clark's graduate students), rather than to the establishment of civil departments of antiquities which marked the pre- and war-time era (e.g., Wheeler in India, and see Paddayya 1995).

World prehistory could more accurately stand accused of scientific imperialism, typified by the establishment of a worldwide mean-time—the 'present' as 1950. The advent of absolute dating enabled the plotting of international spreads (e.g., early man, neolithization) and permitted the rapid development of the subject in regions where hitherto no fieldwork had been undertaken, without the need for arduous typology building linked to foreign ('core') contacts—radiocarbon had made all sequences equal.

A path could be charted linking the global perspective formulated within *World Prehistory* to that of the (one) world archaeology of the 1980s. This would hinge upon developments at the end of the decade in which Clark's volume was first published. It would have to include the establishment of the journal *World Archaeology* (1969), the occasion of two great (shared-editorship) London research seminars, *The Domestication and Exploitation of Plants and Animals* (Ucko & Dimbleby 1969) and *Man, Settlement and Urbanism* (Ucko, Tringham & Dimbleby 1972), and increased interest in overseas ethnoarchaeological study during the 1970s. Yet, apart from the bridging role of certain key individuals, the

interrelationship of Clark's world prehistory and current one world approaches is not straightforward (Clark has certainly not been sought out as the founding ancestor of the present programme).

Inasmuch as the 1986 World Archaeological Congress in Southampton coalesced a number of trends within post-processual archaeology and introduced a critical perspective into the international arena, it stands opposed to the positivist logic which eventually underpinned much world prehistory/archaeology before the 1980s.[14] Such systems-based approaches are, for instance, the antithesis of a contextual archaeology which by definition lacks bridging structure: worldwide multi-national/-ethnic 'voices' risk giving way to cacophony as witnessed in the intentional (inescapable) non-structure of Hodder's (1989) *One World Archaeology* volume. The essential difference between Clark's and the current 'world' project lies in relationship to history and politics. Whereas these factors underpin the present movement, as envisaged by Clark a scientific archaeology of early pre-literate/-*historic* 'man' was intended to encourage common global solidarities, thereby escaping the inadequacy of nationalist history/politics. While celebrating traditional cultural diversity, his situation of the 'ethnographic' or indigenous was essentially timeless. Clark's approach was as much anti-historical as anti-nationalistic.

The aim of this chapter has been to recognize where world prehistory/archaeology has come from. Resolution of the issues upon which its development hinges are not easy: international academic freedom *vs.* coordinated regional/national researches, the subservience of the individual to disciplinary codes and the interrelationship of supposedly universal values with the local (cultural relativism). As witnessed, for example in the fracturing of the Soviet Union and Warsaw Pact nations, today's 'one world' ideals could well be undermined by emergent nationalism and competing ethnicities. There is no pre-determination that sub-national peoples cannot fall prey to the chimera which beset European romantic nationalism—the quest for a purity of cultural origins (e.g., Rowlands 1988). Far from being 'the end of history', the degree of political upheaval in recent years suggests that there is great scope for new others (and, by extension, new 'uses') in the coming decades. Are some indigenous communities to be granted archaeological privilege above others? Who is to set the agenda in one world, let alone two or three, and who will arbitrate the past when ethnic voices collide?

NOTES

1 Because much of this chapter is concerned with conference exchanges (i.e. not with the well-rehearsed phrases of polished texts) extended quotations have been included so as to convey some sense of dialogue and performance—the 'voices' of a time and place: London, 1943–4.

2 While touching upon some of the same issues as the CFA, both *The Future of Anthropology Conference,* held in 1943 to mark the centenary of the Royal

Anthropological Society and the 1944 *Conference on the Problems and Prospects of European Archaeology* appear to have been much less spirited.

The CFA *Proceedings* also offer insights into the conference dynamics of an earlier generation. For example, of the 282 in attendance 62 participated in discussions (22 per cent; 17 per cent if one excludes speakers). In other words, c. 80 per cent of the audience did not debate the future of archaeology. While only three of the 29 papers were given by women, in the discussions women spoke 32 times; men, 45 (42 per cent female participation). Though this seems a remarkably high ratio one must remember that this was 'archaeology at a time of war' (Evans 1989:436–8) and that many of the men, who in peacetime circumstances would have been in attendance, were then away in service (e.g., Piggott, Wheeler).

3 Correspondence from Fl.-Lt P. Shinnie, read in the discussion following Grimes' paper, stressed the need for reasonable fixed rates of remuneration for excavators so as to curtail the current practice of employing the wealthy at the expense of others (CFA: 71). The disdain expressed towards the Civil Service by some certainly suggests an element of class rivalry.

4 The future of post-war archaeology was also discussed in a series of papers in *Antiquity* (1944). It included a review of the first meeting and agenda of the Council of British Archaeology (Anon 1944:158–9); Aileen Fox on archaeology in education (Fox 1944:153–7); Grimes (1944:42–9, 206–8) and Webster (1944:206–8) on museums; and Wheeler (1944:151–2) on the need for a comprehensive programme of excavations to accompany the rebuilding of London.

5 The acknowledgements for the three-month-long excavation of a long barrow at Nutbane, Hants, sums up the conditions of much UK fieldwork prior to the 1970s: 'A grant of £100 was made by the Ministry [of Works] towards expenses, and the Andover History Group, friends and visitors contributed £43. Messrs Dunnings of Weyhill lent wheelbarrows and other tools, and the Girl Guides, through the kindness of Miss Dampier Childe, helped with the provision of tents and equipment…. Apart from four paid labourers in the last few weeks of the excavation, the large labour force was entirely voluntary' (de Mallet Morgan 1959: 15).

Hudson's *A Social History of Archaeology* (1981) is deeply flawed by its extreme advocacy of the UK amateur tradition, reflecting its author's reaction to increased fieldwork professionalism through developer-funding in the later 1970s.

6 His wartime re-evaluation is best appreciated by comparing this chapter with that in the revised second edition of 1947. See also Clark 1938:351; Clark 1939:194–203 (and Evans 1989:440–1) concerning his pre-war attitudes towards archaeology in Germany under the National Socialists.

7 Clark concludes the paper by denouncing 'deculturalization' brought about through industrialism and argues for the appreciation of the diversity of indigenous cultural traditions (Clark 1943:120–1). In the light of this it is relevant that he twice refers to the negative connotations of tribalism using it as an analogue for hyper-nationalism/fascism (Clark 1943:113, 119). While others have also identified a primitive mentality with the herd-like anti-individualism of totalitarian states, it is surprising that Clark (who was to become a leading exponent of the ethnographic analogy in archaeology; Evans 1988) employed such reference. Yet his interests at that time were largely confined to European prehistory and its development was still formulated within a framework of progressive social stages (*From Savagery to*

Civilization, 1946); European 'folk' cultures (i.e., non-tribal peasantries) were the primary source of ethnographic analogy.

8 See Daniel (1950:312) concerning Clark's 'passionate over-advocacy of the place of archaeology in education'.

9 Arguing for the unity of the discipline as a whole, Childe proposed that prehistoric techniques should be applied to historic periods, regional sequences be established around the world and that diffusionism represented 'the pooling of the collective experience of mankind' (Childe CFA: 25). Interestingly enough (as if anticipating criticism of the recent use of the metaphor, 'material culture as text'), in a conference paper of the following year it was the very concreteness of archaeological objects that made them part of 'the common heritage of humanity as a whole' as opposed to literature which loses its 'full perfection' through translation (Childe 1944a:7).

10 In a comparatively recent paper, Clark (1979:1, 14–15) highlighted the threat posed by the 'universalizing character of natural science' *vis-à-vis,* the homogenization and impoverishment of once-diverse traditional cultures through industrialization. Compare this, for example, with his advocacy of science in Clark 1944.

11 At the 1932 London congress non-European delegations only accounted for approximately one third of the participants, many of whom represented colonies (see *Proceedings of the First International Congress of Prehistoric and Protohistoric Sciences* 1934).

12 The fact that, unlike Scotland or Wales, England lacks a national museum and instead has the British Museum, a self-proclaimed repository of 'world culture', is itself a legacy of the nation's once-imperial overseas archaeological interests.

13 The return of archaeologists from wartime overseas service may have also been a factor; world (extra-European) contributions to the *Proceedings of the Prehistoric Society* rose markedly in the immediate post-war years (volumes 11–15; Clark 1959:7, Fig. 1).

14 In a paper of 1968 David Clarke went to far as to condemn the 'localised dogma' of distinct (idiosyncratic') regional/national interpretive traditions, declaring them obsolete in the face of the 'new' international methodology ('analytical archaeology'; 1979:154; cf. Trigger & Glover 1981:134).

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CHAPTER SIXTEEN ARCHAEOLOGY IN RUSSIA AND ITS IMPACT ON ARCHAEOLOGICAL THEORY

PAVEL M.DOLUKHANOV

THE DEVELOPMENT OF RUSSIAN ARCHAEOLOGY

Since its beginnings Russian archaeology has been firmly in the mainstream of European archaeology. As in Europe, archaeology in Russia took shape as part of antiquarianism.

The first archaeological activities in Russia were carried out in the early eighteenth century during the reign of Peter the Great (the excavation of Slavic barrows near Ladoga, the acquisition of golden objects from Siberian barrows, etc.). In 1846 the Russian Archaeological Society was founded in St Petersburg. In 1859 the Imperial Archaeological Commission was set up, attached to the Ministry of the Imperial Court.

From the outset, archaeology in Russia was strongly ideologically biased. Established archaeology in Russia concerned itself with three main areas of research: (1) Classical antiquities; (2) Slavic and old Russian sites; (3) Oriental studies.

For a long time the main activities of Russian archaeologists were concentrated on the excavation of Classical, mainly Greek, sites on the Black Sea coast. No less spectacular were the excavations of Scythian and Sarmatian barrows in the North Pontic region and the establishment of relationships between Scythian and Sarmatian groups with the Greeks. For a long time, Classical archaeology occupied pride of place in pre-revolutionary Russian archaeology. Its spectacular achievements were widely publicized, exhibited in museums and even included in the curriculum of 'classical' *gymnasia*. The official support which Classical archaeology enjoyed in pre-1917 Russia had obvious ideological strings attached. These can be traced back to the sixteenth century 'Third Rome' theory, according to which Muscovy regarded itself as the heir of the Classical (particularly Byzantine) heritage and hence as the spiritual leader of the Orthodox world.

It should be acknowledged that the nineteenth century major excavations of Classical sites amply funded by the state contributed in large part to the elaboration of the methodological principles of Russian archaeology, which were based on large exposures and detailed stratigraphy. The experience gained was

later used in the excavation of Bronze Age sites in Southern Russia, including the famous Maikop barrow (Veselovsky) as well as Pit-Grave, Catacomb and Timber-Grave tumuli (Gorodtsov).

No less ideologically motivated were the excavations of Slavic antiquities of Central and Northern Russia; particularly after 1860 Slavic archaeology in Russia was strongly influenced by Pan-Slavic nationalism. It is significant that at that time several Russian historians expressed the view that the Slavic peoples belonged to a 'new and superior type of world culture, which should eventually result in their unification and dominance over the rest of Europe' (Lebedev 1992: 149). The ideological task of Slavic archaeology in this nationalist context consisted in providing arguments proving how ancient and autonomous initial Slavic settlement in Eastern Europe had been and in demonstrating their high cultural standards and multilateral relations with the outside world. To the credit of Russian archaeologists one should add that the majority of them did not harbour any nationalist sentiments. A number of excellent excavations of early Slavic barrows and settlements were carried out in the late nineteenth century.

Prehistoric archaeology in Russia, as in most European countries, developed largely independently of established archaeology with its romanticistnationalist bias. It is significant that the excavations of prehistoric sites in Russia were initiated by natural scientists who relied heavily on evolutionist paradigms. After the 1870s Russian scholars carried out systematic excavations of Palaeolithic sites at Kostenki (Polyakov 1880) as well as in the Ukraine and Siberia. During these years Neolithic sites were discovered and excavated south of Lake Ladoga (Inostrantsev 1882) and at Fatyanovo cemetery (Uvarov 1881) and the Bronze Age site at Volosovo (Polyakov 1882).

Considerable advances were made in archaeological theory. At the turn of the century Gorodtsov (1901) advanced the principles of typological classification of prehistoric materials which, by a long way, preceded the concepts of numerical taxonomy. Gorodtsov was also one of the first to introduce the concept of an 'archaeological culture' in Russian archaeology. This concept was largely based on the outstanding achievements of Russian archaeology in the late nineteenth and early twentieth century. At that time Russian and Ukrainian archaeologists (Spitsyn 1899; Khvoiko 1901; Gorodtsov 1908; and many others) uncovered and published a great number of known archaeological complexes (such as the Tripolyan one) which completely changed the archaeological map of Eastern Europe.

Shennan (1989) and Klejn (1991) in their discussion of the origins of the concept of archaeological culture traced it back to the mid-nineteenth century. In the words of Klejn (1991:134), this concept emerged in Germany, under the influence of the 'German romantic school of history', which visualized culture as 'the emanation of national spirit' (and see Härke, Ch. 2, this volume). Since that time, this particular school of thought has tended to identify archaeologically distinguishable 'cultures' with distinct ethnicities. This thesis was explicitly formulated in the early twentieth century by Kossinna: 'Sharply defined

archaeological culture areas correspond without doubt to areas of particular peoples and tribes' (Kossinna 1911:3). Although attempts to identify nations with prehistoric artefacts may be observed in Germany from the beginning of the eighteenth century (Malina & Vašíček 1990:62) it is hardly surprising that the wide influence of Kossinna's concept—the main ideological purpose of which was to demonstrate Germanic cultural superiority—coincided with the upsurge of nationalism in Germany on the eve of World War I. The concept of archaeological culture viewed as on a par with ethnicity has had strong nationalistic overtones in Central and Eastern Europe ever since. Malina & Vašíček (1990:64) mention a curious fact: at the peace conference in Versailles after World War I territorial claims to the same territories on the part of Germany and Poland were substantiated by Kossinna and his Polish pupil, Kostrzewski (1923) respectively, each using the same archaeological arguments.

The concept of archaeological culture has an equally lengthy tradition in Russian archaeology. As Mongait (1967:59) wrote, as long ago as the mid-nineteenth century it was noted that 'particular types of artefacts, dwellings and burials tend to cluster within distinct areas, and the assemblages of these finds were referred to as "cultures"'.

Although Gorodtsov (1901), in accordance with his broad deductive strategy, viewed cultures primarily as classificatory units, both he and Spitsyn shared the cultural-ethnic concept which basically equated 'archaeological culture' with ethnicity.

THE ORGANIZATION AND IMPACT OF RUSSIAN ARCHAEOLOGY

On 18 April 1919 Lenin signed a decree establishing the Russian Academy for the History of Material Culture (RAIMK, later GAIMK) in place of the Imperial Archaeological Commission. For more than seventy years the network of archaeological research institutions created in the Soviet Union remained the largest in the world (Trigger 1989). The structure of Soviet archaeology was repeatedly modified in the course of recent decades until it finally reached its fully developed status in the 1970s (Dolukhanov 1993).

At that time two hierarchical levels could be distinguished: All-Union institutions (which were allowed to carry out archaeological investigations in the whole territory of the Soviet Union): research institutes of the Academy of Sciences in Moscow, St. Petersburg (Leningrad) and Novosibirsk. Each of these institutes comprises several departments such as: Stone Age, Central Asia and the Caucasus, North Pontic (Classical) Archaeology, Finno-Slavic Archaeology, Laboratory for Archaeological Technology (St Petersburg), Neolithic and Bronze Age, Slavic Archaeology, Theoretical Archaeology, Archaeological Records, Laboratory for Scientific Methods (Moscow); etc. Each Soviet republic had either its own Institute of Archaeology or Department of Archaeology within an Institute of History. Apart from this, archaeological research was carried out by

the departments of Archaeology of major universities and by national museums (the State Hermitage Museum in St Petersburg, the State Museum of History in Moscow, etc.) Archaeology was either taught as a subject in its own right, or as a subject within history, in local universities. Archaeological collections were housed in museums of history in the capital of each republic.

The second major advantage of Soviet archaeological institutions was their general adoption of sophisticated excavation techniques. Over a period of seventy years Soviet archaeologists excavated a large number of sites from various periods. The excavation technique adopted in the Soviet Union stipulated large-scale horizontal exposures and detailed stratigraphy. Great attention is attached to the identification of various types of structures. The thorough application of this technique resulted in numerous outstanding achievements, including the identification of Palaeolithic dwellings in the 1930s, earlier than anywhere else in the world.

After 1920 Marxist historical materialism became the leading epistemology in the world of Soviet archaeology. In accordance with this approach archaeology was viewed as a part of history generally orientated towards the study of the evolution of society and culture. The Marxist approach was to a large degree responsible for the profound interest shown in sociological interpretation. From that time Soviet archaeologists were to focus on the identification, wherever possible, of the technology, social organization and ideology of past societies.

Both academic structure and Marxist ideology were introduced to Russian archaeology by Nikolai Y.Marr (1864–1934), who was prominent as a linguist even before the Revolution. Basing his ideas on the principles of comparative linguistics, Marr put forward a 'Japhetic theory' of linguistic change, which he regarded as a universal law. After the Revolution Marr tried to link this theory in with Marxism. Languages, he argued, were the product of socio-economic structures and were therefore class-related. The Academy was designed as a huge instrument for the study of the evolution of language in conjunction with all aspects of human material culture. In 1950–1, long after Marr's death, his theories were denounced by Stalin as 'vulgar Marxism'.

In the 1920s heated discussions went on among Soviet archaeologists over possibilities for the reconstruction of socio-economic formations based on the study of 'material remains'. Resulting from these discussions Ravdonikas and other young archaeologists (mostly from Leningrad) announced the creation of a 'Marxist history of material culture' as distinct from the 'old archaeology'. The 'Japhetic theory' was adopted as its Marxist ideological basis.

One of the immediate consequences of the adoption of Marr's theories by the young Soviet archaeologists was the total rejection of the concepts of cultural development which were at that time the prevailing paradigm in the West. Marr's concepts were based on autochtonous development: hence, both diffusionism and migrationism were denounced as 'bourgeois nationalism and racism'.

No more lenience was shown towards the systematic classification of artefacts: this was labelled as 'naked artefactology' (Bulkin, Klejn & Lebedev 1982:288–90).

Ravdonikas viewed as the basic unit for historical-archaeological analysis

> a cultural complex constituted by the entire material culture of a concrete society at a given moment of its development in its concrete originality and in the integrity of all interlinked elements of culture and other accountable social phenomena…[Archaeological analysis should imply] the restoration of all kinds of production and production relationships, all aspects of everyday life and ideology, thus resulting in the perception of a complex of material culture as a vivid fragment of a vivid social whole.
>
> (quoted in Lebedev 1992:428)

In the 1930s Soviet archaeologists put forward the 'stadial concept' which viewed prehistory as a sequence of changes regarding 'socio-economic formations'. This concept also stemmed from Marr's 'Japhetic theory', according to which all languages passed through identical stages of evolution. These were equated with 'socio-economic formations'.

Starting out from the stadial concept, Efimenko advanced a theory in the 1930s according to which the primordial horde was the basic social structure which occurred in the course of the Lower and the Middle Palaeolithic. This was replaced by the matriarchal clan society with the transition to the Upper Palaeolithic. The remains of Palaeolithic 'long houses' were cited as one of the arguments in favour of such an interpretation.

Krichevsky (1940) viewed early agricultural (Tripolye) society as corresponding 'to the flowering of the matriarchal clan and 'primordial communism'. Passek saw in the late Eneolithic Usatovo cemetery indications of social stratification and accumulation of wealth, reflecting the transition to patrilineal organization (Passek 1949). Kruglov & Podgayetsky (1935) linked Bronze Age collective tombs with communal ownership and individual barrows with patriarchal pastoral societies.

Oddly enough the strong emphasis on Marxist sociology often coexisted in early Soviet archaeology with the culture-ethnic approach inherited from Gorodtsov and Spitsyn. The concept of an ethnic-related archaeological culture was never totally abandoned in Soviet archaeology. Thus Bryusov (1956:20), who became prominent in the 1930s, wrote: 'Archaeological cultures thus understood reflect in their unity, originality of technique, the economy, way of life and other aspects of life of a defined ethnic group, usually groups of related tribes, in their specific historical development'.

Marxist social concepts in early Soviet archaeology attracted much interest among archaeologists in the West. Gordon Childe was the most consistent and the most active in this respect. Childe's interest in Soviet-style Marxism was

primarily dictated by his quest for a materialist conception of history. This interest was strengthened after his first visit to the Soviet Union in 1934.

Childe's socio-economic interpretation of the Three Ages stemmed largely from the stadial concept:

> accordingly the archaeological division between the (Thomsen's) Three Ages provides no serviceable basis for a subdivision of Barbarism into stages. Consequently our Soviet colleagues during the 1930s abandoned not only Thomsen's old division but also any attempt to find a better technological basis for classifying archaeological cultures. Instead of Palaeolithic, Neolithic, Bronze Age and Iron Age, they spoke of 'pre-clan society' *(dorodovoye obshchestvo),* clan or gentile society *(rodovoye obshchestvo),* and class society *(klassovoye obshchestvo)*.
>
> (Childe 1951:27)

Childe was consistent in his Marxist approach: his knowledge of Marxism was not confined to Marx and Engels and Soviet Marxist writers, but also embraced Lenin and Stalin (this can only be understood against the background of the attitude towards the Soviet Union prevalent among western left-wing intellectuals in the 1930s and 1940s). Yet more important was the fact that Childe was for a long time the main channel via which Soviet-style Marxist archaeological concepts reached the west.

Largely due to Childe's influence, the Marxist-oriented Soviet archaeology of the 1930s and 1940s had an enormous impact on the archaeological thought of the twentieth century. We can distinguish two major paths via which these concepts were disseminated: (1) conceptual diffusion promoted to a large extent through Childe's works; (2) direct indoctrination of archaeology students in Communist-block countries.

The emergence of the New Archaeology in the 1960s and 1970s, the most outstanding archaeological phenomenon after World War II, may be seen as the most obvious example of the earlier trend. If one compares the main theoretical premises of the New Archaeology with the principles advocated by Ravdonikas (see above), the similarities are clear. This was noted both by Russian and American writers. Klejn, who is generally hostile both to Soviet pre-war archaeology and the New Archaeology, writes:

> there is the same pious approach to theory, the same passion for generalizing and abstracting causes in the cultural process to the detriment of an interest in concrete historical events (what we later called 'sociological schematism'), the same disregard for delimited comparative typological studies ('simple or formal artefactology'—to use the language of that time), the same striving for functional definition and the interpretation of phenomena as a complicated complex, the same fiery negation of migration and diffusion, the same indifference to ethnic

> boundaries and the same insistence on contrasting itself with traditional archaeology.
>
> (Klejn 1977:13)

Trigger bases a similar conclusion on the following observations:

> Both approaches were based on an evolutionary view of culture change and sought to understand the regularities exhibited by that process. They agreed that these regularities were strong and could be studied by using a materialist frame-work. Migration and diffusion were played down in favour of trying to explain the changes that occurred within cultural systems over a long period of time. Traditional typological studies that sought to elucidate chronological and spatial variations in material culture were regarded as old-fashioned and there was a corresponding increase in functional intepretations of archaeological data.
>
> (Trigger 1989:326).

Given the similarity in their main theoretical concepts, both the Soviet archaeology of the 1930s and the New Archaeology may be viewed as a single paradigm (Kuhn 1970).

It is highly significant that the Soviet archaeological theories of the 1930s, through Childe's mediation, gained recognition in various parts of the archaeological world. As Tsude (Ch. 14, this volume) notes, all Childe's major books were immediately translated into Japanese, and Childe's (i.e., Soviet-Marxist) 'historical-materialist logic' for explaining social evolution has been adopted by many archaeologists in Japan. Tsude also stresses that Marxism continued to play a dominant role in Japanese historical (and archaeological) studies until the late 1960s.

In this connection one should also note the existence of a para-Marxist movement in social thought which still exerts an appreciable influence on certain archaeologists in the West. Proponents of this movement in archaeology stress the complexity of modes of production, the importance of human consciousness in bringing about change, and, in particular, the major significance of clashes of interests and conflicts in classless societies (Trigger 1989:339). In many respects these studies repeat investigations undertaken, and conclusions reached, by their Soviet colleagues in the late 1920s and 1930s, without their authors realizing they are doing so, investigations which were later denounced as 'vulgar Marxism'.

As for the second trend (indoctrination), it is necessary to point out that after the 'victory of socialism' in the countries of Central and Eastern Europe, in China, North Korea and Vietnam, both archaeological university training and archaeological institutions were organized on the Soviet model. Huge 'Institutes of Archaeology' (often under different names) are still in existence in practically all these countries (Bökönyi 1993; Gringmuth-Dallmer 1993; Neustupný 1993; Schild 1993; Velkov 1993;). All university students were obliged to study

Marxism and were influenced by it. Consequently Soviet-style Marxism and Soviet-style archaeological theory became the only permissible ideology of archaeology both in Eastern Europe and China (Zhang Chi, 1992). At the same time one should stress that in the mainstream of Soviet Marxism a considerable number of highly original archaeological theoretical works were published, particularly in East Germany, Poland and Czechoslovakia (Malina & Vašíček 1990:139–40).

It is very interesting to follow the impact of Soviet archaeology on the countries of Asia. Russia traditionally took a considerable interest in Far Eastern studies, at least from the beginning of the eighteenth century, when the Russian scientific missions started studying Chinese historical sources (e.g., Bichurin, 1777–1853).

This interest was considerably enhanced with the establishment of the Peoples Republic of China in 1949. A.P.Okladnikov, a leading Soviet archaeologist specializing in the prehistory of Siberia and Central Asia, set up a special research group in the Institute of Archaeology (Leningrad) to study Sino-Russian cultural contacts. This group included V.E.Larichev, a Sinologist with wide interests in archaeology (e.g., Larichev 1969, 1972) and, slightly later, M.V.Vorob'ev a specialist in Korean linguistics (Vorob'ev 1974, 1975).

Soon after the establishment of the Siberian Centre of the Academy of Sciences in Novosibirsk (1957), Okladnikov became the director of its Institute of History, Philology and Philosophy. Larichev and several other archaeologists became members of this Institute, carrying out studies on Chinese and Far Eastern archaeology. At the same time, Vorob'ev moved to the Institute of Oriental Studies in Leningrad (St Petersburg).

During this period a number of prominent Soviet archaeologists (notably Professor P.I.Boriskovsky) undertook prolonged study tours in China, studying archaeological collections and lecturing on various aspects of Soviet archaeology. At the same time, many Chinese students were studying in Soviet university departments of archaeology and anthropology. These were years of active penetration by Soviet-style Marxism into Chinese archaeology. Consequently, Soviet-style Marxism and Soviet-style archaeological theory became the only permissible archaeological ideology in China (Zhang Chi pers. comm.).

After the deterioration of Sino-Soviet political relations during 1962 and 1963, and particularly during the 'Great Proletarian Revolution' of 1967–76, archaeological contacts between the Soviet Union and China came to an end. Many Chinese Soviet-trained archaeologists lost their jobs and influence. While retaining its Marxist character, archaeology in China acquired a nationalist bias reflecting the country's new ideology of self-reliance. A number of Chinese students who sought refuge in the Soviet Union during the Cultural Revolution continued their researches into the prehistory of China and Japan, notably in Novosibirsk (Chzhan Ya-Tsin 1973; Chan Su Bu 1973).

Meanwhile, a group of archaeologists and orientalists, based in the Institute of Oriental Studies (Leningrad) and in other institutions, carried out systematic

studies on the prehistory and early history of Korea (e.g., Gafurov 1974; Pak 1974).

Intensive archaeological and ethnographic investigations have been carried out by Russian expeditions in Mongolia since the 1870s (Przhevalsky, 1839–88; Kozlov, 1863–1935). Systematic archaeological fieldwork there was intensified with the establishment of the Soviet-Mongolian expedition in 1949, headed first by Okladnikov and later by Derevyanko. Major discoveries have ranged from early prehistoric to Turcic medieval sites (Okladnikov 1973; Derevyanko 1981). At least in part aimed at counterbalancing a growing Chinese cultural offensive, such fieldwork was particularly well funded in the 1960s and 1970s. During this period, Mongolian archaeology was largely shaped after the Soviet model, with all leading Mongolian archaeologists having been trained in Soviet or eastern European universities.

Boriskovsky was the first to 'open up' Vietnam to Soviet archaeology. He carried out a series of archaeological investigations in the 1970s, first to North and later (after 1975) also to the former South Vietnam. Those carried out jointly with Vietnamese archaeologists in Hanoi led to the discovery of a number of Palaeolithic sites (Boriskovsky 1971). Following the signing of a Treaty of Friendship and Cooperation in 1978, a number of agreements were entered into between archaeological institutions in Vietnam and the Soviet Union covering all branches of pre- and early history. Large numbers of Vietnamese students were trained in Soviet universities, some then publishing major works—adopting standard Soviet methodology and interpretative frameworks—on various aspects of Vietnamese prehistory (e.g., Nguen Van Rien 1972).

More recently there have been large Soviet archaeological and ethno-graphic expeditions to Afghanistan, Iraq, Syria and Yemen (People's Democratic Republic). In some cases these expeditions, which have always included local specialists, have resulted in spectacular discoveries (notably in northern Afghanistan and Iraq). That in the former Peoples Democratic Republic of Yemen (Aden), the largest Russian mission abroad, includes the study of prehistory, Classical archaeology and the Middle Ages, as well as ethnographic, linguistic and anthropological investigations. Although several Yemeni archaeologists and anthropologists have been trained in Russia, no visible influence from Soviet-style Marxism is visible in the archaeological publications of this and other of the Middle Eastern countries.

Archaeology developed differently in various former Soviet republics. The Ukraine had strong archaeological traditions before the October revolution: Kiev, Odessa and Kharkov were important archaeological centres in pre-revolutionary Russia; Lvov was an important centre of Polish and Ukrainian culture first in Austria-Hungary, later in Poland. Between 1929 and 1938 the national archaeologies of the Ukraine, neighbouring Bielorussia and other 'national limitrophe areas' suffered severe losses; many archaeologists were arrested and executed, being accused of 'bourgeois nationalism'.

Tiflis (Tbilisi), like Kiev and Odessa, was an important archaeological centre in pre-revolutionary Russia. In the 1930s and 1940s national Academies of Sciences (which included archaeological institutions) were set up in the three Transcaucasian republics (Georgia, Armenia and Azerbaidjan). They included archaeologists of both the old generation and the new 'national cadres'. Many leading archaeologists of Russian origin were actively involved in the archaeological investigations of the Caucasus carried out at that time.

Archaeology was not really established in the Soviet republics of Central Asia until after the Revolution. During the 1920s and 1930s archaeological enquiry in those areas was mainly undertaken by Russian archaeologists. Gradually, young 'national cadres' trained in the universities of Moscow and Leningrad took over. Now, there are Institutes of Archaeology (or Departments of Archaeology) in all Central Asian republics, headed by local archaeologists. Today, there are important Russian archaeological expeditions, several with the active participation of western archaeologists, working in these areas, particularly in Turkmenistan and Uzbekistan.

The Baltic States (Estonia, Latvia and Lithuania) which gained independence from Russia in 1918, developed their own important archaeological centres. After their annexation by the Soviet Union in 1940, many of their local archaeologists left the country, some of them becoming prominent in the West. After World War II, archaeology in the Baltic Soviet republics conformed to the Soviet model. Despite considerable ideological pressure, Baltic archaeologists have now succeeded in re-establishing local archaeological schools primarily based on pre-war traditions.

As Klejn notes (Klejn 1993b:65), the national archaeologies of the former Soviet Union were constantly manoeuvring between a 'Scylla of indictment in nationalism and a Charybdis of submission to Russification'. Any such manoeuvring had to be accompanied by lip service to Marxism, which was the only permissible ideology.

One of the consequences of the ideological domination of Soviet archaeology within the national republics was the widespread acceptance of the cultural-ethnical approach. In the real context of actual politics such an approach was often naive: thus, local archaeologists tended to identify their own nations with the 'glorious ethnicities' of the past (Klejn 1993b: 67). For example, Armenian archaeologists often equated present-day Armenians with Urartians; Azeri archaeologists saw the origin of distinctive Azeri ethnicity in the antiquities of Caucasian Albania; Moldavian character was often identified with that of the Dacians, etc.

These are not just harmless academic exercises. For example, there is a long-lived rivalry between Georgian and Armenian archaeologists, both asserting claims to an 'Urartian heritage'. Similarly, the cultural and political significance of the Turcic expansion is interpreted very differently in each of the neighbouring Central Asian republics. In the course of the All-Union archaeological conference in Baku in 1985 Akhundov, an Azeri archaeologist,

argued that well-attested Armenian stone crosses *(khachkars)* found in Nagorno-Karabakh were in reality Albanian monuments—Caucasian Albania is regarded as the cultural precursor of modern Azerbaijan. It is significant that these particular debates actually preceded open hostilities which flared up much later. Kohl (1994) cites this, and many other examples, to demonstrate that archaeological interpretation (still based within a cultural-ethnic framework) in the former Soviet Union was, and still is, often made use of in ethnic conflicts in order to substantiate particular claims to disputed territories.

ARCHAEOLOGY IN RUSSIA TODAY

As Lebedev (1992:431) notes, the theories of Marrism ceased to be applied in Soviet archaeology in the early 1940s. It was officially 'liquidated' after a series of Stalin's articles (Stalin 1950). At the same time, there was a gradual retreat to the leading pre-revolutionary cultural-ethnic paradigm (Lebedev 1992:431).

The stadial theory was abandoned in the 1960s to 1970s. Grigor'ev was one of the first to denounce it openly. He argued that no major social differences are detectable throughout the Palaeolithic period. The nuclear family had existed as early as the Lower Palaeolithic: he sees no evidence for clan organization in the Upper Palaeolithic (Grigor'ev 1968). In his latest book Lebedev (1992) agrees with another Russian archaeologist, Anikovich, who labels the stadial theory 'Stalinism in archaeology'.

In the post-war period the Marxist approach took the form of 'sociological archaeology' developed mostly on the basis of materials relating to early agricultural civilizations of Central Asia and the Caucasus (Masson 1976; Alyokshin 1986; and others).

The study of prehistoric technology culminated in the works of Semenov and Korobkova (Semenov 1964; Semenov & Korobkova 1983), and their followers, who were the pioneers in the development of the principles and the practical implementation of use-wear analysis.

For many years Soviet archaeologists were involved in endless discussions about the subject-matter of archaeology. Zakharuk (1978:53) took as the prime subject-matter of archaeology the comprehensive reconstruction of cultural assemblages (both material and spiritual) of past peoples, with the aim of 'modelling past societies as functional social entities'. Hening's (1983) objectives were similar.

Masson (1990:6), however, adopts a somewhat different position, seeing archaeology as the study of regularities in the evolution of cultural objects (in their 'artefactual-productive forms') and their structures (cultural, economic, social and ideological).

Another school of thought, often referred to as 'strict archaeology', focuses attention on the development of the theoretical basis of archaeology and on the strict definition of its essential concepts (such as 'type', 'assemblage' and 'culture'). Following the empiricist approach advocated by several western

archaeologists (such as André Leroi-Gourhan and Robert Dunnel) they seek to separate archaeology from the social sciences. As Klejn (1993a:347) writes:

> history and archaeology are different disciplines and their fusion is harmful for both.

The different approaches of Soviet archaeologists to archaeological phenomena became particularly evident in the treatment of 'archaeological cultures' which are almost universally recognized in Russia as the basic archaeological concept. Discussion focused on the nature of the content of such 'cultures', and how they should be interpreted. The cultural-ethnic approach which stemmed from Spitsyn's and Gorodtsov's theories remains the leading paradigm for numerous Russian and Ukrainian archaeologists. Thus, Bryusov (1952:20) wrote:

> Archaeological cultures thus understood reflect in their unity, originality of technique, the economy, way of life and other aspects of life of a defined ethnic group, usually groups of related tribes, in their specific historical development.

Zakharuk (1964:39), an Ukrainian archaeologist, equated archaeological cultures with linguistic entities:

> An archaeological culture is an aggregation of chronologically and territorially interrelated archaeological sites (complexes) of a defined type, which reflect the territorial diffusion and stage of historical development of a group of related tribes speaking dialects of the same language.

Another Ukrainian scholar, Braichevsky (1965:51) went still further when he wrote:

> We regard archaeological culture as the association of archaeological phenomena which correspond to certain ethnic entities. We cannot recognize as a culture an assemblage which does not correspond to a definite ethnic entity.

Based on the direct equation of archaeological cultures with ethnicities, Soviet archaeologists developed theories relating to the origins of the Slavs (Lyapushkin 1961, Artamanov 1974, Sedov 1982) and of the Scythians (Artamanov 1974; Grakov 1974).

Another group of scholars tended to view archaeological cultures as purely taxonomic units. Such a perspective can be traced back to Zhukov (1929), who saw an archaeological culture as an assemblage of typical elements, an approach echoed more recently by Sorokin (1966:5):

> Archaeological culture is an entity of archaeological sites which form the stable complexes of similar attributes and which are distinct from other complexes of attributes forming different archaeological cultures.

In recent years, particularly since the political collapse of the Soviet Union, there has been an almost complete abandonment of Marxist interpretations of archaeology. Some writers complain that this had led to a methodological vacuum in which 'it is difficult to work' (Kozlov 1992). As a possible solution, Russian archaeologists and ethnographers often refer now to the theoretical legacy of Russian non-Marxist ethnologists and historians, such as Bakhtin, Propp, Rostovtseff, Klyuchevsky (Tishkov 1992; Nosov 1993). One of the dangerous side-effects in Russian post-Soviet archaeology today (as has been seen above) is the rise of nationalism, which is usually associated with the ethnic-cultural approach. It should not come as a surprise, therefore, that Lebedev in his latest book (Lebedev 1992) views the ethnic-cultural approach exemplified by Kossinna (and see Härke, Ch. 2, this volume) as one of the highest achievements in archaeology, superior to the evolutionary paradigm.

In recent years, books by Gumilev on ethnogenesis have become particularly popular in Russia. These writings, where literary phantasies replace serious analysis of hard evidence, are often openly racist (e.g., Gumilev 1990).

CONCLUSIONS

Until 1917, archaeology in Russia was developing along the lines of European archaeology, with a pronounced bias towards the cultural-ethnic paradigm. Marxist-orientated archaeology, which developed in Russia in the 1930s and 1940s, had a profound impact on modern archaeological thought. Today, Russian archaeology is characterized by the abandonment of the Marxist paradigm in favour of the resurrection of the cultural-ethnic approach, often with strong nationalist overtones.

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CHAPTER SEVENTEEN
WHERE ARE WE NOW?
Archaeological theory in the 1990s

JULIAN THOMAS

ON DISCIPLINARY IDENTITY

> In most departments the syllabus remains doggedly devoted to regional and chronological approaches to archaeology with little emphasis on thematic or generalised approaches…. In some places, I suspect, the teaching of courses on the history, methods and principles of archaeology carries a low priority and low status, is regarded as a bore, and is inflicted on the junior members of staff.
>
> (Champion 1991:140)

As Champion points out in his succinct discussion of recent theoretical developments in British archaeology, there is some contradiction between the unusually animated debate which has taken place over the past two decades and the relatively minimal impact which it has had in some quarters. From at least the time of David Clarke onwards, theoretical works emerging from the United Kingdom have had an international currency (see Clarke 1972a). It could be argued that this influence has been all the more profound since 1982 and the emergence of a range of semiotic, anthropological and sociological approaches to material culture whose initial gestation took place in Cambridge (Tilley 1989a: 185). Yet, as Champion implies, the interest in these developments has been patchy in British universities and virtually nonexistent in public archaeology. In the context of the present volume, it is equally significant that the extent to which archaeologists in the United States have been exercised by the debate over 'post-processual archaeology' (either positively or negatively) (Earle & Preucel 1987; Preucel 1991) has not been matched in other parts of the world (see Andah 1995; Funari 1995; Mackie 1995; Politis 1995; Paddayya 1995). Understanding the predicament of archaeological theory in the United Kingdom involves two distinct issues: the character of the questions being debated, and the nature of the institutional context within which archaeological knowledge is produced. However, as demonstrated below, these two sides of the problem are not unrelated.

The recent history of British archaeological theory can perhaps be characterized as one of discontinuity in terms of the specific problems being addressed, underlain by a deeper continuity of process. Yoffee & Sherratt (1993:2) identify this continuity in the way that 'post-processual' archaeology has chosen to search for social theory outside of the discipline itself, in much the same way as the New Archaeology did. It is arguably the case that the processual/post-processual debate has been distinguished by a certain degree of mutual parody and misunderstanding (Edmonds 1990:23), and also by a desire to establish clear lines of demarcation between rival 'schools'. Yoffee & Sherratt are thus correct in their assertion that the reality is less clear-cut than the rhetoric would sometimes indicate. But if recent developments merely carry forward past trends, it is all the more difficult to suggest that they constitute an aberration. It is perfectly possible to argue that many contemporary philosophical positions in British archaeology represent the culmination of a process set in motion by Clarke and his contemporaries, who clearly advocated that archaeologists should pursue an eclectic course in their search for inspiration (Hammond 1979:5). What has changed is that rather than being restricted to contacts with the natural sciences, archaeology has been opened to the range of debates which are current within the human sciences as a whole. Consequently, much of the recent literature in the United Kingdom has concerned issues as diverse as post-modernity and post-modernism, interpretation and politics, relativism and rationalism, power and authority, text and context. Several of these debates have involved the erosion of essentialist positions, and a critique of totalization. A decade ago, these might have seemed very unusual topics for archaeology: doubtless to many they still do. However, this does not mean that the discipline has been 'hijacked', or that it has lost its sense of direction (a conclusion which seems to be implied by Yoffee & Sherratt).

Many of these problems can be attributed to the vision of disciplinary identity to which one subscribes. In the first place there is a 'territorial' model, in which all possible knowledge has been divided up between a discrete set of academic disciplines, and is segmented by borders which are jealously patrolled by the ontology police. Here, we stand in Archaeology-land, but if we start to contemplate the 'wrong' subjects we might inadvertently stray across the border into History-land or (perish the thought!) Literary-theory-land. By contrast, we might choose to consider the academic disciplines as 'traditions of inquiry'. In these terms, the production of knowledge is a social process, in which the disciplines define (and create) their own objects of study (Foucault 1970, 1972). Hence there is no pre-given field of knowledge which was at some point defined as 'archaeology' and handed over to the archaeologists, who would henceforth act as its custodians. Rather, a self-reproducing community of academics has defined particular issues as being their particular concern. Over a period of centuries, this community has come to represent a tradition, within which new investigators have been nurtured. Thus, because I have been trained within a particular community, consider particular issues to be of importance (the nature

of culture, the significance of material culture, the character of the European Neolithic…), and acknowledge particular disciplinary ancestors (Lubbock, Childe, Piggott, Clarke…), I am an archaeologist. I might make frequent forays into other areas of knowledge in order to find conceptual morsels, but I invariably scurry back with them to the disciplinary nest.

The discipline, then, is a significant element in the social context within which knowledge is produced. It imposes particular rules and standards which must be adhered to if we are to be heard and recognized. Much as recent innovations have sought to extend the boundaries of accepted discourse, there is always a line which must not be crossed, and part of what is acquired through inculcation into the tradition is an ability to judge where that line lies. Many of the ideas which are now finding their way into archaeology have come from philosophy, anthropology, geography and literary theory, and many of the debates in which archaeologists are involved have always been interdisciplinary. Theory is nomadic, but none the less it is transformed by the context in which it is deployed, in relation to the set of concerns which it is used to address. Consider the uproar which greeted Hodder's 1982 publication, on the grounds that it represented an importation into the discipline of perspectives which were unscientific, humanistic, untestable and generally wishy-washy. Yet precisely the same approaches had caused similar reactions in literary circles for diametrically opposite reasons: the spectre of the 'universal science of the sign' threatened dehumanization, a disrespect for the canon, and the death of Leavis's 'sensitivity' to the text. Archaeology is not alone in fearing that it represents a 'consumer' of (perhaps inappropriate) theories which originate in alien disciplines. It is instructive to look back to Gregory (1978), which discussed the significance of Marxism, phenomenology and critical theory in overcoming the positivist orthodoxy which existed in geographical studies:

> If the discussion which follows appears to be insufficiently 'geographical'… this is only because geography is perhaps the last of the social sciences to take the claims of critical social science seriously.
>
> (Gregory 1978:78)

Exactly the same could be said of archaeology, and there is no doubt that similar disclaimers could be found in recent radical works of history, literary theory, philosophy, religious studies, education, and so on. This being the case, one is inclined to ask where all of this theory comes from. One answer might be that new perspectives are often thrown up at the interstices of the established disciplines, perhaps through the jarring of ideas which originate in different contexts: precisely the effect of the 'importation' which seems to be so threatening to some. Consequently, many of those who have been responsible for work in these areas have been notoriously difficult to pin down. Was Foucault a philosopher or a historian? (His chair, in 'History of Systems of Thought', had to

be invented for him.) Was Barthes a man of letters or a linguist? Is Kristeva a semiotician or a feminist literary historian?

There is little to be feared in entering into dialogue with other disciplines, with finding out what they are saying and asking whether it has any relevance to the concerns of archaeologists. If the result is that archaeologists transform their ideas of what it is to do archaeology, then so much the better. The allied and very real danger is acceptance of a set of theory as a package, 'off the peg' as it were. This is perhaps most likely where a particular structure of ideas has been formulated as an inclusive system, a 'theory of everything'. This might apply to frameworks as diverse as sociobiology (Wilson 1975), chaos theory (Gleick 1988) and structuration theory (Giddens 1984). This is not to deny the utility of those approaches (or not all of them, at any rate!). Rather, the point is that archaeologists have to work with theory. No extraneous system of thought can simply be 'applied' to archaeological evidence, any more than archaeologists could ever construct a theoretical edifice for themselves which would henceforth need only to be learned and put to work. Theory needs to be constantly renewed, so that archaeologists must be skilled in theory-building as much as in data retrieval. Moreover, the broader political and cultural context in which the work of archaeologists is undertaken is by no means stable. It would be unrealistic to imagine that archaeology might be untouched by the social upheavals which have occasioned major reconsiderations of the human sciences in general over the past thirty years (Gouldner 1970). One corollary of recognizing that theory is transformed in relation to context is that one might be less than enthusiastic about seeing Anglo-American archaeological thought transported intact into the Third World. Here, indeed, we might be conspiring in a form of academic colonialism (e.g., see Andah 1995). Ideas are altered by the geographical setting in which they are employed: take, for example, Marxism, which has managed in the past thirty years to represent simultaneously an inflexible and repressive orthodoxy in Eastern Europe, a complex academic discourse in the West, and an inspiration for popular liberation struggles in Latin America. Similarly, the most impact that one would hope that western archaeological theory would have in the rest of the world would be as an object with which to engage, in the production of knowledges which are locally appropriate (and see several chapters in this volume).

'POST-PROCESSUALISM': A SPECTRE HAUNTING BRITAIN?

It has become conventional to group together those archaeologists who have adopted positions which relate to the critical human sciences under the rubric of 'post-processual archaeology' (Hodder 1985). While there is something to be said for this label in identifying a broad critique of positivist empiricism in archaeology, it would be inappropriate to hypothesize the existence of a post-processual 'school'. The degree of unanimity among these individuals is far less

than would have been found among the practitioners of, say, the American New Archaeology at a comparable stage in its development. As Hodder (1991b:37) has recently suggested, it might be more accurate to talk of a post-processual *phase* in the development of Anglo-American archaeology. Indeed, it may be the lack of a recognizable 'party line' which partly explains why the presumed movement has failed to influence archaeologists in almost all parts of the world (see several chapters in this volume). Ironically, Yoffee & Sherratt (1993:5) raise as one of their criticisms of 'post-processualism' the way that previous generations of archaeologists have been dismissed with blanket generalizations. Certainly, the polemical statements of Binford (1965) or of Watson, LeBlanc & Redman (1971) should not be construed as representing an homogeneous 'processual' orthodoxy. Yet Yoffee & Sherratt perform exactly the same manoeuvre as that which they criticize, in erecting a parodic rendering of 'what post-processualists think'. Thus, for example, they state that:

> explanation in post-processual archaeology is the process of deciphering the meaning-laden constitution of material culture.
>
> (Yoffee & Sherratt 1993:5)

While this might indeed cover the approach advocated by Hodder (1986), Barrett (1988:6) clearly considers that the 'translation' of past cultural texts is a hopeless task, whilst Olsen (1990:199) indicates that what an artefact 'meant' is in any case less important than how it allowed (multiple) meanings to be produced. Another point which clearly divides the supposed members of the post-processual school is the status of the human subject: Hodder's focus on the 'active individual' (Hodder 1986:6) as opposed to Shanks & Tilley's (1987a: chapter 3) radically anti-humanist account of the formation of the subject, or Barrett's (1987) concern with the contextual emergence of subjectivities. By asserting that some of these approaches to identity neglect gender and implicitly assume a male subject, Gilchrist (1991) and Engelstadt (1991) place a further cast on the issue.

If it is to be argued that the past decade has seen the emergence of archaeology as a social science rather than the consolidation of a unified school of 'post-processual' thought, it remains to be considered why this development has been resisted in some areas of the discipline. One possibility lies with the extent to which theory is recognized as having an applicability to the practical aspects of archaeology. Hodder (1991a:8) argues that it was principally the methodological concerns of processual archaeology which resulted in its having some influence upon field archaeology (e.g., Payne 1972, Cherry, Gamble & Shennan 1978). However, this was sometimes not matched by a commitment to the explication of past social processes. In many cases the combination of processual methodology and the 'rescue' field ethic (record it now, consider what it means later) has resulted in an understanding that excavation reports should consist of exhaustive data treatment and analysis, but that these should not necessarily be

used as a basis for interpretation. By contrast, contemporary theoretical archaeology is all the more concerned with interpretation, yet is reluctant to present a 'cookbook' guide to 'how to interpret your site'. In some quarters, this is seen as precisely its failing: an inability to formulate a systematic and transferable method.

None the less, it would be inaccurate to suggest that social scientific archaeology has been concerned with philosophical issues to the exclusion of any engagement with the empirical. Significantly, several prominent works have recently emerged which make use of ideas drawn from the human sciences as a means of working through bodies of archaeological evidence (e.g., Hodder 1990; Tilley 1991). This may reflect a growing recognition that many in the discipline are more easily convinced by example than by theoretical argument, and also perhaps demonstrates a certain weariness with having continually to rehearse arguments which were won a decade ago. In Kuhnian terms, it may be that archaeology as social science has reached its stage of 'normal science'. Moreover, from the mid-1980s onwards it has been possible to discern the existence of a number of younger archaeologists, less exclusively concentrated in Cambridge, who have gained some degree of inspiration from the original 'post-processualists'. Among this generation, however, ideas drawn from various forms of social theory have generally been seen as a means of addressing a given problem in prehistory or proto-history (often in the form of a Ph.D. thesis) (e.g., Richards 1988; Foster 1989; Hill 1989; Moreland 1990; Kirk 1991; Edmonds 1992; Johnson 1993). While this work has largely been pitched at the level of synthesis, the significance of archaeology as a reflexive social science has begun to be turned back upon the means by which our evidence is produced (Richards in press; Tilley 1989a). The recognition that field archaeology is a social practice, whose precise form (democratic or hierarchical, 'objectively' recording or problem-orientated) directly conditions the outcome of the exercise, may in time have more fundamental consequences than the methodological developments of the 1970s.

Were it to be more widely accepted that archaeology is theoretical and interpretive at every level of its operation, it might be hoped that a more democratic structure for the discipline might emerge. No archaeologist is ever 'just' a digger or 'just' a laboratory technician: they have an active role in constituting evidence and its interpretation. It is for this reason that all archaeologists need to be skilled in the conceptual as well as the technical aspects of their subject. Clearly, this has consequences for the way that archaeology should be taught in universities, but as Champion indicates in this chapter's opening quotation, this is not a view which is widely held. It is worth dwelling for a moment upon why this should be the case. One might imagine, to judge from the alarmist tone of some of the literature, that some aspects of contemporary archaeological theory were on the brink of upturning the established order of the academy. Certainly, feminism, Marxism, hermeneutics and critical theory all imply a degree of reflexivity. From these perspectives,

archaeology is not considered in a vacuum, but as a social practice embedded in relationships which are implicitly or explicitly an object of critique. However, there is little sign of the imminent collapse of the established *status quo,* or even that radical critique is having to be met with savage authoritarian repression. In truth, the critical archaeology which, from New Mexico or Arizona, might appear to herald the end of civilization as we know it, represents only a very small part of academic archaeology in the United Kingdom, and it has ever been the case that theoretical archaeology was only practised by a minority.

THE PLACE OF THEORY IN THE ACADEMY

In order to place the issue in perspective, an admittedly crude pencil sketch of British university archaeology is germane. Within the 26 departments of archaeology distributed across the United Kingdom, there are rather more than 200 archaeologists who hold academic appointments (Austin 1987:233). It is obviously unrealistic to attempt to generalize regarding the philosophical views of individuals whose positions might change from week to week. However, as a rough estimate, it might be suggested that over 100 of these have an outlook broadly in keeping with that which Wylie (1993:21) characterizes as 'traditional'. That is to say, they are largely concerned with the extraction, description, classification and compilation of archaeological evidence relating to a particular period, or amenable to a particular kind of scientific analysis. As Wylie indicates, traditional archaeologists are frequently 'millennarian' in their approach, believing that it is their task to accumulate as exhaustive and well-documented a record of their chosen material as possible, on the understanding that when complete this record will constitute a self-evident account of past human activity. This is not to deny that many who might be dismissed as 'atheoretical archaeologists' implicitly operate sophisticated conceptual approaches to the past (Freeman 1991:103). But it does at least indicate a reluctance on their part to engage in theoretical discussions within or beyond the discipline. In contrast, one might estimate that there are somewhat less than 40 individuals whose approach to archaeology is broadly 'processual': influenced by the American New Archaeology or the Cambridge palaeoeconomy school. That is to say, for these persons, archaeology should be carried out as far as possible as a hypothetico-deductive science, whose objective is the establishment of generalizations concerning human behaviour. In many cases (but by no means all), it is asserted that human beings should be understood as one element in a broader ecosystem, and that their behaviour is best considered using the same analytical framework as for other living systems (O'Connor 1991). One might then suggest that a further group of around 30 persons hold a range of positions which are strongly theoretical, but who would reject the suggestion that their work was 'post-processual' in character. This would include marxists, feminists, devotees of the Annales and a range of other eclectic combinations of social theories. Importantly, many of these would accept that archaeology has a

significant political and cultural role. However, their relationship with those identified as 'post-processualists' varies between sympathetic fellow-travelling and open hostility.

Finally, it remains to identify those archaeologists who might arbitrarily be labelled as post-processual. As indicated above, these are a very disparate grouping, whose internal differences are as great as those which distinguish them from other groups. For the sake of argument, though, let us take as criteria for inclusion the acceptance of a central place for a politically related critique of the discipline; a concern with the language in which archaeology is constructed (rather than merely reflected), and with writing; a willingness to continually re-evaluate our own analytical concepts; a rejection of the possibility of final and definitive accounts of the past; an insistence upon the nature of archaeology as a social practice carried on in the present; a recognition of a plurality of perspectives; a rejection of positivism, empiricism, scientism and naturalism. This being the case, there are probably fewer than 15 persons concerned. Consequently, it is barely surprising that this strand of British theoretical archaeology has so far failed to transform its parent discipline and its teaching, either at home or abroad.

Academic archaeology in Britain is thus overwhelmingly empiricist in tenor. It would probably be unfair to suggest that the majority of its practitioners view theory with either disdain or mistrust. However, it is possible to distinguish a widespread misconception of what theory is and what it does. To the empiricist, archaeology is understood as a series of technologies for the extraction and treatment of data. Archaeology thus forms its own root metaphor, in that all of the operations of the discipline are conceived in terms of the progressive uncovering of a hidden but otherwise self-evident truth. Excavation, the archaeological practice of disclosure *par excellence,* is the arena in which positions of authority are constructed in British archaeology (*contra* Kohl 1993: 18). It is the excavation monograph which demonstrates beyond question the professionalism of the archaeologist, and which provides access to positions of power: chairs, committees, learned societies. Consequently, it is frequently the case that other archaeological practices are understood as allegories of excavation, as technologies of disclosure. Thus, for instance, scientific techniques are comprehensible as excavations in miniature, discovering more facts within the laboratory. Similarly, a range of other techniques (lithic technology, ceramic analysis, draftsmanship, field survey…) are considered as parts of a battery of approaches which can be used to develop an ever more complete picture of the past. In this scheme of things, theory is one more specialism, to be conducted by one more kind of specialist. Just as with neutron activation analysis, it is unnecessary for most archaeologists to understand what goes on inside the black box, but if the end result is interesting it is to be tolerated.

Hence the place occupied by theory in British university archaeology is determined by a category error. Theory is widely considered to be something which a *theory specialist* 'does' to data. In keeping with a spirit of liberal

pluralism, each university department should have one theory specialist, just as it should have one lithic specialist, one ceramic specialist and one environmental specialist. Students can then be offered a range of options, and decide whether they wish to become a theoretical archaeologist, a lithic archaeologist…. In these terms, the objection that all archaeology involves embedded assumptions, employs linguistic codes and carries implicit political and cultural values, in short, that all archaeology is theoretical, is considered profoundly illiberal. 'Are you saying that everyone has to do archaeology in *your* way?' Yet this is to claim no more than David Clarke did twenty years ago when he wrote that 'whether we appreciate it or not we always *operate* controlling models in the interpretation of observations' (Clarke 1972b:3).

WRITING, RELATIVISM AND FRAGMENTATION

Since reality is experienced in a multiplicity of different ways by persons who are positioned according to a range of factors (geographical and spatial location; cultural heritage; gender; class; differential access to information; and so on), it is difficult to argue that any one person's account of their experience is definitive. Similarly, since historians or archaeologists are themselves socially and temporally located, their account of the past must be seen as a partial and provisional one (Gadamer 1975; Foucault 1984). It is this concern with the archaeologist as a social being in the present, who produces an understanding of the past on the basis of prejudice, assumption and imperfect knowledge, which has lead to a concern with *writing* as such (Shanks & Tilley 1987b:18). However, the suggestion that archaeology should relinquish its claims to a privileged access to a 'true' past has caused some disquiet within the discipline.

> Archaeologists today cannot afford multiple versions of the past to proliferate…. It is critical that archaeologists assert that there is at least a partly knowable antiquity and that archaeologists are the guardians of its integrity.
>
> (Yoffee & Sherratt 1993:7)

Yoffee & Sherratt's volume, which originates from a Theoretical Archaeology Group conference session intended as a critique of 'post-processualism', demonstrates some of the problems which arise from such an essentialist position. On the one hand, there is an assertion of a real, knowable past. Yet in the same book there is Kohl (1993:15) insisting upon the primacy of the material evidence, such that we can only place any faith in past realities where they are directly documented by 'hard facts'. So the past is knowable, but this knowability is subject to limitations. Unfortunately, as Wylie (1991:32–3) demonstrates, this evidential distinction between what can and cannot be known leads us into a position where the existence of women in the past is only accepted where it is 'proven' by osteology or iconography. If archaeologists have a

professional responsibility, is it to say things to the public about the past which are guarded, dry and authoritarian, or as human and as interesting as they can honestly be made? It may be the case that the things which really interest archaeologists about the past (for instance, social organization: Yoffee 1993) are not empirically knowable. This, surely, is the crux of the problem of archaeology as social science: that which archaeologists seek to investigate is not a collection of material things or even an assemblage of behaviours which are standardized from place to place and time to time. Human beings operate in a world which is always already interpreted and culturally constructed (Shennan 1989:14). An understanding of past social worlds depends upon interpretation and the construction of coherent arguments. Yet as soon as the evidence is interpreted, the possibility of competing hypotheses emerges, which may not be simply 'right' or 'wrong' so much as emphasizing different aspects of reality.

A similar concern over relativism and the erosion of a 'real' past appears to motivate Engelstadt's (1991) critique of Shanks & Tilley. Engelstadt sees in post-processual archaeologies (which she associates with post-modernism) the danger of a disruption of any stable historical identity, and hence the reality of any feminine presence in the past. Where nothing is fixed, there can be no ground for a definitive account of the past, and 'the practice of deciding between a plurality of meanings then seems to become a matter of power' (Engelstadt 1991:505). One might object, however, that the notion of an essential (and biologically determined?) female identity which this seems to imply is not particularly helpful. On the contrary, the recognition that gender is (by definition) culturally constructed does no damage to the reality of the experience of *living as a woman*. It is precisely this lived experience which has led many feminists to dispense with any requirement for a unified and definitive understanding of the world. Indeed, it has been from feminism that the most compelling critiques of totalized knowledge have emerged. Deutsche (1991), for instance, has characterized the desire to draw together all aspects of a social situation, to constitute them as an object and to gaze on them from a distance as a distinctively male way of appropriating the world. It constructs a 'dream of unity' in the eye of the god-like observer.

This need not be taken as an argument for a totally relativist position, in which there is not merely no sovereign truth, but no reality either. A middle position is possible, which could be referred to as *perspectivism*. What this implies is that there is an external reality, which is both hugely complex and spread across enormous periods of time. Human beings will only ever experience a fragment of this totality, and their attempts to encompass even a small part of the whole in knowledge and language are gross simplifications. Thus there was also a real past, and we do experience real traces of past persons, yet our understanding of that past must be one which is imperfect. Perspectivism implies that while reality exists *out there,* we apprehend it from a perspective, and that our understanding will be one among many. Consequently, our knowledge is always incomplete, there is always more to know about the past and the present, and there will be no

final point at which we have achieved a definitive understanding (Hodder 1991b: 37).

On this basis, Hodder (1991a:15) sees the *partially* objective character of the past as a means of distinguishing between the identity claims of ethnic minorities and the views of 'fringe' archaeologies. However, this does not obviate the political character of interpretation. It may be the case that the 'truth' of a particular understanding of the past (as defined by western archaeology) is not the only consideration to be taken into account (and see Paddayya 1995). Thus one might wish to uphold the right of 'New Age Travellers' to ground their identity as a social group upon an understanding of Stonehenge which one personally considers to be erroneous (even if only to a degree more so than that presented by English Heritage!). Or one might not wish to eradicate an Australian Aboriginal understanding of certain sacred places as having been created in the Dreamtime, despite the evidence of scientific dating. Yet one might equally wish to contest interpretations of the Balkan past which serve as a basis for practices of 'ethnic cleansing'. These models of ethnogenesis might or might not actively misrepresent the archaeological evidence: in all possibility a sparse record might prove highly malleable. In these circumstances, the question of whether or not the evidence has been misrepresented can only constitute a first line of defence. Rather than accept a total relativism in which 'anything goes', however, archaeologists have the responsibility to make choices: they have to discriminate. While Engelstadt (1991) is doubtless correct that in making these choices archaeological knowledge is deeply connected with social power, it is the role of critique to attempt to unmask this power. Thus the evaluation of competing pasts is not merely a matter of identifying the perspective from which they are written: it demands consideration of the effects which a given interpretation will have, and the uses to which it might be turned (Thomas 1990).

Such a perspectivist outlook will also require that the concepts which are employed in such analysis be regarded as provisional. One of the drawbacks of the macrosociological approaches to the past which descend from Marx and Weber has been a 'realism of the concept', in which heuristic devices are gradually awarded the status of real entities. Thus, while notions like that of 'mode of production', 'social relations' or 'chiefdoms' have proved useful in the investigation of particular social and historical contexts, in other circumstances they might prove restrictive and inappropriate (Baudrillard 1988; Weiner 1993). Continually evaluating the conceptual toolkit is one more aspect of the critical perspective which is essential to archaeology. It is all too easy to allow an elegant concept to swamp the detail of the evidence. It has been this concern with the inflexibility of generalized models of social process and social change which has led many archaeologists to argue against prehistories which are written at the global and supra-regional level (e.g., Barrett 1989). From this point of view, that which is overgeneralized is less secure and more removed from the evidence. An alternative is an archaeology which is written 'from the bottom up', beginning

with the details of local circumstances and the rhythms of everyday life. For Sherratt (1993:125) this concern with the local holds the danger of a myopic overemphasis upon 'isolated regional instances', thereby missing the large-scale trends. Equally, Marxists might argue that too tight a focus on the small scale and too much suspicion of our own modes of analysis can breed political paralysis. For them, it would be of paramount importance to maintain a grasp on 'the big picture' as a basis for action. However, it can be argued that Wylie's (1993:24) metaphor of 'tacking back and forth' between interpretations and evidence could equally be applied to the local and the pan-regional perspective. Thus we should consider the existence of large-scale and long-term processes, but primarily in terms of the ways in which they were lived through and experienced at a human scale (Thomas 1994).

WRITING THE PAST: SOME FURTHER CONSIDERATIONS

As written, the past is produced by the contemporary labour of the archaeologist. Consequently all archaeology is ultimately literature, and is susceptible to literary analysis. As has often been pointed out, the past can be written about in a number of different ways, which seek to convince the reader by distinct strategies (White 1973). Ricoeur (1984) argues that the past can be constructed as Same, as Other, or as Analogue. That is to say, the traces of the past can be presented in such a way as to be familiar and comforting, or as alien and disconcerting, or by the combination and juxtaposition of the two we can seek to expose the character of our written text as an artifice: something which 'stands for' the past, but which is only ultimately an interpretation. Elsewhere (Thomas 1990), I have argued the case for a past written as the Other, a past which can act to destabilize and de-legitimize the present through its alterity. However, Cixous (1986) has pointed out that where binary oppositions are established which are in some way related to the division between Same and Other, it is generally the case that some evaluation is implied. That is, such oppositions are hierarchical. The classic example would be that between Man as Same and Woman as Other, so that Woman becomes an outsider, an aberration from human culture, and Man is the norm. Such oppositions are not merely accorded unequal value, but involve a struggle and a violent repression of the Other (Shiach 1991:7–8). Yet at the same time, it is the polarity between Same and Other which sets up the relationship of desire. While a past which is written under the sign of the Same bears the danger of convincing us that things are 'just as they ever were' (human beings have the same desires, needs and habits as in prehistory), this account tells us something of the potential danger of a past written as Other.

If the past is the Other of contemporary society, it becomes something which must be both desired and dominated. A popular desire for the past is nostalgic and romantic, such that a series of modernist ideals can be projected onto it. At the same time, the past as Other can be something whose distance holds a particular

kind of comfort, convincing us that all is well with the present. Hence our relationship with the past, a relationship of desire, is governed by a complex erotics (Shanks 1992). In the sphere of the Heritage Industry, the elaborate nature of our eroticized relationship with the past can be demonstrated by a visit to Warwick Castle in the British West Midlands. There are at present two permanent displays within the castle, one concerned with instruments of torture, and one a reconstruction of a royal wedding at the turn of the present century. Here desire and repression are closely connected. The collection of torture irons allows us to imagine states of physical extremity, clearly connected with the sexual, but in a distanced and vicarious manner. The tools are not in use, and the perverse pleasure of looking is made respectable by being sanctioned as 'history'. Moreover, this is a 'safe' pleasure, because it concerns the past, a past construed as 'the bad old days'. Conversely, the 'royal wedding' plays not on the sexual thrill of a dangerous past, but the yearning for a past of elegance and fairy-tale beauty. Here there are wax figures of princesses and dukes, surrounded by sumptuous material goods, yet held together as members of an ideal family. These are recognizable named individuals who lived in the past, and their involvement in the 'universal' family event of marriage allows us to identify with them. This is how it would have been for us, if we had lived in the past. Each member of the family, including the smallest of babies, is identified by a label giving name and social rank. The servants, however (including the woman holding the royal baby), are anonymous, and we are not encouraged to consider the nature of *their* lives.

What is happening here is that the categorical division between past and present allows us to consider the former as an externality: something which has no necessary connection with the present. Where the past is objectivized it ceases to be seen as being constitutive of the present. As Cixous (1986:111) has it:

> In a certain way the father is always unknown. Coming from outside, he has to enter and give proof. Outsiders, absolutely other, strangers, ghosts, always capable of coming back…Coming out of the earth to go back into the mother, into the palace, to reappropriate bodies and goods. That is what is called civilisation.

Cixous' prose, with its references to Freud, Oedipus and Electra, is obscure here, but catches some points which are of cardinal importance. We do indeed consider past people as strangers and ghosts, precisely because they 'come from outside' and from out of the earth. In the western world the past is outside, and this spatial metaphor demonstrates its being 'somewhere else'. Our dead are always absolutely Other because being in the past they are in something which has been defined as an object. In this sense they no longer constitute ancestors, distinguished by their 'having-been-ness'. In constructing the past as an object (something in which archaeology is deeply implicated), we alienate ourselves from it. This severance and alienation is what allows the past to become the

focus of an erotics, since it is the gap between past and present which established the potential of desire. By definition, this is a gap which can never be filled: desire is endless and beyond fulfilment. The irony of our post-modern relationship with the past is thus that while we are fixated with its horrors and pleasures, which remain always out of reach (even if we 're-enact' them), we have ceased to be rooted in the past. Thus the past is thought about, analysed, circulated as a set of commodities, but less often simply felt. Alienation and fragmentation into bundles of 'general knowledge' allows the establishment of an *economy of pasts*.

Some have chosen to see the commodification of the past as being bound up with the 'depthlessness' and 'difference' of post-modern theories (Jameson 1984; Walsh 1990). This would seem inherently unlikely, since the actually existing condition of post-modernity can hardly be conflated with any movement defined as 'post-modernism'. On the contrary, if our culture is one which now conceives of past and present as radically separate then one way forward is through modes of thinking and practices of writing which are frequently stigmatised as 'post-modern'. In the first place, we require a more sophisticated understanding of time than one which sees a sequence of 'nows', like the frames of a film, one of which is the present and the others being divided into past and future. This demands that we separate the 'time of the world' (Ricoeur 1988), the time which can be measured on clocks or by radioactive decay, from human time. By human time we might mean the sense of having-been, being alongside other beings, and projecting oneself forward into the future which is absolutely fundamental to human identity (Heidegger 1962). Being a human being is essentially temporal, in that we could not conceive of ourselves as persons, who have personal histories and who formulate projects for the future, if we could not distinguish between past, present and future: we would simply disperse. While in one sense we perceive these categories as absolute, in another they are inextricably linked. What makes humans human is that they *care* about their own existence: in Heidegger's terms, they are beings whose own being is an issue for them. It is the structure of *care* which holds together past, present and future.

In human terms, the past is not something which has happened, and the future is not something which is going to happen at some indeterminate point which has yet to arrive. On the contrary, past, present and future *are*. The past *is,* in the present, because I have been, I have memories, and I can discover traces of the having-been-ness of other persons (that is, archaeological evidence). Similarly, I can say that I have (not 'will have') a future because I project my plans into it, even if I can not predict any of the details of what is going to happen to me. Therefore, past, present and future are unified in the structure of human consciousness. They cannot be adequately expressed in the spatial metaphor of different places. While we would not wish to deny that real and independent processes go on in the world, and are stretched across world-time (the seasons, ecological succession, geomorphological change), we as humans can only recognize these changes because of our temporal constitution. If there were no

human beings, these processes would go on regardless of our absence, and yet our ability to gain an analytical knowledge of world-time events is based upon our more basic experience of human time.

The recognition of the human character of temporality implies the need for a reconsideration of the relationship between past and present. A past which is 'objective' (and by definition alienated) is in some senses less authentic than that which is the past of a person or group of persons. Such a past, characterized by its 'belonging' to someone, can nonetheless make no claims to being definitive. Many such subjective pasts can be entertained, and will have an important role in the establishment and maintenance of personal and ethnic identities. These pasts are human, positioned and fragmented: they are important, and yet none of them is *the* past. As far as the archaeologist is concerned, this might suggest that our writings should take on a character somewhat similar to the 'polyphony' advocated by the so-called post-modern ethnographers (Clifford & Marcus 1986; Marcus & Fisher 1986). Many different situated accounts of the past might then be placed alongside each other. This would not, however, overcome the serious criticism which has been raised against polyphonic writings, that it remains one particular person (the anthropologist) who orders, sanctions and effectively controls the final text.

In dealing with the similar problem of how women can convey their own experience within a discourse which is dominated by male rules of practice, Hélène Cixous advocates what she terms a 'bisexual' form of writing (Shiach 1991:16). That is to say, she recognizes that to simply establish a 'female' textuality in opposition to male discourse is to implicitly accept the finality of the male/female division, and with it the subordinate value accorded to women and their works. Instead, she attempts to write on both sides of the divide, employing both male and female modes of expression, and thereby seeking to undercut the vicious gap between the two. This practice may provide a lesson for archaeology. For the archaeologist there presently appears to be a stark choice between modes of textuality. Over many decades, the discipline has developed a sanctioned means of writing about the past which employs a complex series of disciplinary codes: the archaeologist is always absent from the text; the evidence is presented in a manner which is assumed to be both objective and universal in its application; particular areas of knowledge are placed in the hands of 'specialists', who are accorded authority in their own sphere but who are encouraged to refrain from making general statements; empirical observation is awarded a priority over theoretical hypothesis, yet the assumptions inherent in observation and description are not considered; and so on. Synthesis thus takes the form of gathering together 'facts' which have been generated by this method. Following the argument already developed, the drawback of such a writing is not merely that its objectivity may prove to be illusory, but that it actually facilitates the alienation and commodification of the past.

In opposition to this, a number of experimental forms of writing have begun to emerge: texts which attempt 'empathy' with past persons (Hodder 1986:95) or

which make use of the evidence to 'tell stories' which seek to humanize the past (Spector 1991; Tringham 1991:124); accounts which openly adopt the perspective of a particular social or ethnic group (McDonald, Zimmerman, McDonald, Tall Bull & Rising Sun 1991); the use of collage, montage and techniques of defamiliarization as means of disrupting traditional narrative (Shanks 1992:188); works which draw attention to the conventions of standard academic writing in the hope of challenging them (Hodder 1989); and attempts to resist totalization through a focus on the human scale and human experience (Thomas 1993). All of these attempt to grasp a past which is 'owned' rather than objective, and fragmentary rather than definitive. Following Cixous, one might attempt to write in two different tropes: the normalizing and objectifying archaeology of the Same, and the fragmented, disruptive archaeology of the Other. By juxtaposing the two, a different kind of writing is achieved, Ricoeur's (1984) 'Past as Allegory', in which what is written comes to be recognized as something which *stands for* the past. Our written discourse is disclosed as an imperfect and provisional understanding: that which is constructed according to the academic code is too dry and lifeless, that which is written from commitment and passion is too partial. By 'closing the gap', a delicate balance is struck between the twin dangers of making the past too distant from us (alienation) and allowing it to become too close (blind faith).

CONCLUSION

It could be conjectured whether or not the establishment of archaeology as a social science represents one more Kuhnian paradigm shift, which will duly be replaced when the next fashion comes along. However, an alternative interpretation is that archaeology has reached a watershed in disciplinary maturity. This is not to suggest that a fixed set of ideas will dominate archaeological thought for the foreseeable future. But it may be that the modification and replacement of those ideas will take place as a part of the development of the human sciences as a whole. However, such a future for British archaeology is by no means inevitable. The adoption of new approaches is only partly a function of the persuasiveness of the arguments being put forward, and is largely conditioned by the networks of power which constitute the discipline. Presently, archaeological theory is a minority activity in British universities, and this position is maintained by its perception as a specialism, rather than as a prerequisite for undertaking any form of archaeological activity whatsoever. The status of archaeological theory as a 'ghetto' area within the academy will only be broken down if the complementarity of theory and practice are recognised. Here there is certainly reason for optimism, as more work emerges which contradicts the image of abstract introspection which is still promoted by some. However, in the long term the concern should be for future generations of students, and whether they will have the opportunities, the

institutional support and the funding which will be necessary to develop a mature archaeology.

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CHAPTER EIGHTEEN
FRENCH TRADITION AND THE CENTRAL PLACE OF HISTORY IN THE HUMAN SCIENCES

Preamble to a dialogue between Robinson Crusoe and his Man Friday

LAURENT OLIVIER and ANICK COUDART

Does French theoretical archaeology exist? This question was raised recently by one of us (Coudart, in Cleuziou, Coudart, Demoule & Schnapp 1991) in an article which attempted to define the position occupied by French research in relation to the main currents of thought or major debates rife in the discipline over the last thirty years. The position of French archaeology *vis-à-vis* the processual and post-processual schools of Anglo-Saxon archaeology was obviously one of the major preoccupations. In particular Coudart sought to bring out the features in the structure and history of the discipline in France which might explain why the Anglo-American archaeological 'theories' had never really found their place in the French archaeological debate.

With this in mind the article tried to distinguish the original contributions of French research and laid special emphasis on the long-standing interest in the culture of techniques. The origins of these ideas must be sought in the philosophy of the Enlightenment, and particularly in Diderot's and d'Alembert's *Encyclopédie,* in which the social (and so also the political) role of technology predominates (Diderot & d'Alembert 1751–65). In this chapter we go more deeply into the issue of the nature of French research, and outline what seems to us to be the originality, or rather the contribution, of the French tradition to the 'theoretical' problems which are debated in Anglo-American archaeology.

FRENCH SPECIFICITY: 'TOTAL HISTORY' WITH A SOCIAL DIMENSION

What makes the French approach specific in the domain of the human sciences is the central place accorded to history. It is the social dimension of history which plays a primordial role, and which aims to study, beyond the sequence of events, the interaction of humans with the specificity of nature and with the diversity of other cultures. In a continent with a long history (continental Europe, in this case) the transformation and reproduction of identities imply necessarily taking into account cultural confrontations and lasting geopolitical structures. Geography, cultural differences, politics and history lie at the heart of every event. This close meshing of space, time and human identity was bound to produce, in continental Europe, mentalities different from those which an island tradition has

engendered (since here the construction of a shared history has always presupposed the rejection of other cultures) or from those in a country with a short history or a colony (since here an interest in prehistory implied primarily an interest in the natives of that country). Febvre (1953:32) gives this definition of 'total' history, firmly anchored in the present, open to allied disciplines, and interested in all forms of social expression:

> It is the scientific study of the various activities and creations of men in the past, spotlighted at the moment they happened, within the framework of societies, which, though very different, are yet equivalent to each other… and with which they covered the earth's surface and the succession of ages.

This plan for a 'New History' around which developed the *Annales* school towards the end of the 1960s calls for two basic comments. First, this global history includes explicitly the fields of historical and prehistoric archaeology, which cannot be split off from it in any way. Archaeology is an absolutely integral part of history. Second, in its orientation, this 'New History', which strives to elaborate a new 'Science of Man' and is based on a truly ideological foundation, breaking with positivist dogmatism and traditional event-based history, has echoes of another revolution, that of the "New Archaeology" of the Anglo-Americans. Here we find the same striving towards 'total' research, the same desire to break with traditional studies, the same desire to adopt a scientific approach. The fact that this trend towards the renewal of the human sciences in France has sprung from history and not from archaeology probably explains why this movement has not experienced, as was the case with the Anglo-Americans, the shift from a 'processual' current to 'post-processual' interests. This was because the structural or 'scientific' *explanation* and the contextual or 'historic' *understanding* were intended, from the start of the 'New History' programme, not to be in opposition, but rather to illuminate one another. This is also surely why the stakes in the struggle between the 'processuals' and the 'post-processuals' have never been fully appreciated in France, and why the term 'New Archaeology' continues to be used to denote both currents, since, from an ideological point of view, they are two different expressions of the same thing.

If we turn now to the situation of 'theoretical' archaeology, as it is practised in Europe (Hodder 1991), we are struck by three fundamental points. The most obvious is the absence of a commonly accepted definition of what is meant by a *theory* of archaeology (Olivier 1992). In most cases the terms of theoretical archaeology are used more or less synonymously with those of processual or post-processual archaeology. However, this synonymity cannot be taken for granted. A theory is defined above all as an *essential* representation of reality, produced by means of a formal procedure, inside which the initial observation of the real world and its final theorization can be verified one against the other. We are still a long way off this situation in archaeology, where the production of

ideas must not be confused with that of theories. The second characteristic of the status of this 'theoretical' archaeology in Europe is the fact that the perspectives of Anglo-American research carry little weight in the preoccupations of the various archaeologies on the continent. These remain dominated by the desire to highlight what is essentially a national heritage, as they have been ever since they were founded. The third point (which follows from the first two) is the predominance of archaeology's role in the construction of national identities and the overwhelming influence in most of Europe of the tradition of cultural history *(Kulturgeschichte)* attached to Germanic archaeology (Härke 1995). Once again, the weight of history has a significant effect of the trend of the debate within the discipline.

THE PROBLEM OF IDENTITY: ONE OF THE MAJOR THEORETICAL CHALLENGES OF ARCHAEOLOGY TODAY

After the fall of the Berlin Wall and the end of the Cold War the crucial theoretical question of archaeology today is that of national identity, or more specifically that of the relationship archaeology enjoys with the construction (or the fabrication) of collective identities. In fact, since research first began, the essential role of archaeology has been to work to reappropriate the past, to attempt to mend the splintered mirror of our origins. Just as Freud, at the time of the 'rediscovery' of the ruins of Troy (Kuspit 1989), had the very same insight, archaeology literally gives body to the myth of origins, by revealing its material presence, buried almost intact under the weight of the debris of the past. These thousands of vases, pieces of jewellery, weapons and tools collected in museums are the accumulation of the material proofs that this original past, on which the collective identity is based, really did exist.

For the sake of claims to this collective identity today people are slaughtering one another in Bosnia or Croatia, the former Soviet Union is on the brink of disintegrating into small states ravaged by civil war, and xenophobia is emerging again in Europe. The list of aggressions is long, almost interminable. All these conflicts, all these exclusions have one point in common: the fact that the expression of collective identity no longer goes by nation-state, but by the recognition of 'ethnic' minorities which now form, through their juxtaposition, what was understood before by nation. The nation-state has exploded, and this phenomenon does not affect only the eastern bloc countries: it concerns the western democracies as well and the United States, where the state is less and less able to guarantee the safety of all its citizens. Obviously archaeology is called on to play a significant role in this process: by resurrecting cultural genealogies (to aim to prove, for example, that the Baltic states were never Slav), by restoring demarcation lines, with the aid of distribution maps of types of remains (to try to show, for instance, that Macedonia has always been Greek) (Marcos 1992), and by giving back to those who were stripped of them by states

now discredited, their cultural heritage, proof of the validity of their ancestral values.

Seeing the world split between the processuals on the one hand and the post-processuals on the other is simply unrealistic. If the world of archaeology is to be divided at all, it should be shared, in Europe at least, between a handful of evolutionists and a horde of anti-evolutionists. The basic issues are quite simple: the evolutionists believe that the identity of a system—that is to say in the case which interests us here of a 'culture' or a 'people'—does not exist in itself, but rather is defined fundamentally by the interaction of the system with its environment, and that it is shaped by the series of events it experiences. The anti-evolutionists will have none of this. For them the identity of a culture or a people exists in itself. It is transmitted by itself from generation to generation, or is transformed by the diffusion of other cultures, which too are defined *per se*. This theoretical divide has methodological implications as well: ever since the last century it has split prehistoric archaeology, evolutionist by nature, from most of historical or Classical archaeology which tries to reconstruct the cultural identity of contexts documented or mentioned in historical texts. Finally, this split is clearly geopolitical in nature. It isolates one fringe of the discipline, linked to the sphere of influence of the natural sciences, resisting the nationalist arguments, from an academic tradition devoted to concerns of cultural history, often favoured by authoritarian regimes, and embodied in the Germanic school since the end of the last century (see Härke 1995; Politis 1995; Funari 1995; and Jorge 1995).

The interpretation of history, the analysis of the way in which history shapes society and, conversely, the way the collective identity is fed by history, are obviously at the heart of the problem, and the French approach has much to contribute to the debate. One of the major preoccupations of archaeology over the next couple of decades will be to find out whether the discipline, confronted by the re-emergence of these questions of identity, will be able to evolve a language of its own. In other words, will it be able to develop a theoretical basis? Or, as it has done up to now, will it drift with the fluctuating ideas of collective identity, from the *racial state* of the inter-war period to the *ethnic minority state* of the turn of the century?

THE INVENTION OF HISTORY

History is the new frontier of the sciences which were formerly called exact. No longer do they function in a world where time, always repeated, always constant, was the basis of experimentation, of the repeatability of observations, on which scientific demonstration is built. From now on the universe has a history, matter has a history, the reproduction of living things has a history, and mathematics itself describes systems transformed by their own history (Gould 1989). This change in the paradigm breaks down the boundary between the so-called 'hard' and 'soft' sciences, and renders obsolete the debate on the specificity of the

human sciences with reference to scientific proof. On the other hand, this reversal of the theory invites us to consider afresh the historical concepts used in the disciplines applied to the past of human societies.

For the idea of *history,* which is one of the basic concepts of research today, came in with the ferment of ideas introduced by the philosophy of the Enlightenment. From around the end of the eighteenth century the identity of things and beings was no longer determined as belonging to a series of fixed categories into which all natural phenomena had to fit, as in the classical order. Now their identity was defined within the movement of time, their *history:* position in time, not place within a grid, gave meaning to the different parts making up a phenomenon and linked them inside a chronological process. This change is at the origin of the basis of most contemporary disciplines. They depend on taking into account time as an essential dimension of the identity of scientific phenomena: history, but also philology, biology, political economy (Foucault 1970:217–21) …and now, as has been mentioned, genetics, astrophysics, the chemistry of dissipative structures or non-linear mathematics.

By drawing attention to supposed ethnic features, the nationalist uprisings, from the second half of the nineteenth century onwards, have obscured the universal and social aspects of the concept of history invented by the Enlightenment. If history is what gives societies and systems their special features, if events shape evolution, this is also because each of the constituent elements of the society, each *individual* possesses in him/herself the basic potential to develop, in other words s/he is also basically *free*. As Coudart (1993) has shown, one way of acting on the system has been devolved onto the individual units of the system by the system itself. For if a society is coherent and can work only from one cultural model, this cannot be stable or uniform. The outline of each of its component parts is shaped by a whole scale of culturally defined variations, which allows each unit to act—as an individual—on and in the totality of the system. Besides, the nation cannot be reduced to any one group perpetuating the same traditions, the same beliefs or the same ethnic allegiance. The nation, like natural systems, is composed of free individuals whose cooperation makes up the social body or structure of the system, and the fate of the system is to develop, not to stay the same. Similar elements of a system or members of a society have basically similar opportunities to develop, that is to say that they are in themselves basically *equal*. This equality runs through all the various systems or societies, and applies to all people, of all nations, of all social origins, but also of *all the times* in which different forms of social grouping followed one another. Thus Rousseau ([1753] 1994:26) writes:

> Thus, (…)! shall suppose him (man) to have been at all times formed as I see him today, walking on two feet, using his hands in the same way we do.

Further on, writing about the great travellers, who in the course of their wanderings met the most primitive races on Earth, he speculates (Rousseau [1753] 1994:109) that if they then began to write:

> the natural, moral and political history of what they had seen,…we would see a new world grow under their pen, and so we would learn to know our own.

In these few lines are set out the basic principles of what will turn into the disciplines of prehistory and anthropology (Lévi-Strauss 1973). Prehistory will be the knowledge of humans through natural origins of society as revealed through the primitive races. Anthropology will be the awareness of the universality of mankind through the diverse forms of social grouping, whether stratified or not. The one will not exist without the other. Prehistory and anthropology will be criss-crossed by similar problems, as they spring from the same dream, the same regret for the loss of Nature: the dream of a state of original harmony, where Nature's bounties belonged to all, a dream supplanted by the violence of the present age, which is devoid of meaning, and in which prosperity is the result of the domination of those who have everything over those who have nothing. Anthropology and prehistory derive their meaning from the same approach as history. For, as Foucault (1970:376–7) puts it:

> and yet ethnology itself is possible only on the basis of a certain situation, of an absolutely singular event which involves not only our historicity but also that of all men who can constitute the object of an ethnology…: ethnology has its roots, in fact, in a possibility that properly belongs to the history of our culture, even more to its fundamental relation with the whole of history, and enables it to link itself to other cultures in a mode of pure theory.

THE QUESTION OF HISTORY, NATURE AND THE MYTH OF ROBINSON CRUSOE

The Enlightenment laid down the theoretical foundations of what constitutes today the human sciences and at the same time it showed up the rift or contradiction between *nature* and *culture,* the two basic terms which serve to define our *modern* identity (Latour 1993). The concept of *total history* as well as the interest in the study of social functioning are rooted in this revolution of ideas, lying behind the idea of a *'Science of Man',* postulated periodically from the end of the nineteenth century to the present day. Archaeology and anthropology are destined to play their part fully in this global approach, which opens on to the whole range of disciplines surrounding them. For the debate which began at the end of the eighteenth century by inventing the concepts of our identity still fuels the questions at the centre of contemporary research. How

is history driven and how are systems changed over time? Are these changes transitory or vital? What is nature and what is culture and how do they interact in the social phenomenon?

The eighteenth century, when modern thought took shape, and which, in the words of Foucault (1970:385) 'serves still as the positive ground of our knowledge', has bequeathed us a book, which gives us the ideal model of the organization of knowledge with an historical dimension and also a novel which is entirely devoted to the central question of the relationship between culture and nature. The book is *Encyclopédie* by Diderot and d'Alembert, and the novel is Defoe's *Robinson Crusoe*. The story of the return to nature in Robinson Crusoe exerted a strong influence on Rousseau when he came to write Emile, his treatise on natural education:

> I hate books. They only teach one to talk about what one does not know…
>
> Since we absolutely must have books, there exists one which, to my taste, provides the most felicitous treatise on natural education. This book will be the first my Emile will read. For a long time it will alone comprise his whole library, and it will always hold a distinguished place there. It will be the text for which all our discussions on the natural sciences will serve only as a commentary. It will serve as a test of the condition of our judgment during our progress, and so long as our taste is not spoilt, its reading will always please us. What then, is this marvellous book? Is it Aristotle? Is it Pliny? Is it Buffon? No. It is Robinson Crusoe.
>
> (Rousseau [1762] 1991:184)

Marx, for his part, saw in Crusoe's experiences on his island a model of a self-sufficient economy (Marx 1954:76–7). *Robinson Crusoe* is in fact a myth for modern times (Watt 1975). The shipwreck cuts a man off from the world to which he belongs, and brings him face to face with his own identity, torn between the pull of nature and the weight of his own culture, already that of a productive society. The years of enforced exile, outside time and far from civilization, provide the fragile frame in which opposites fall away and the dream of a state of harmony between nature and culture can be realized at last. *Robinson Crusoe* is a distillation of all the fantasies and all the contradictions of our modern identity. As Rousseau pointed out, it is a work central to the acquisition of any knowledge and one which illuminates all the questions posed about the world which lies outside our own culture.

In the following pages one of us (L.O.) has imagined a sequel to the discussion begun in the eighteenth century by Crusoe and Man Friday, between the European burdened by all the contradictions of his society and the native, who belongs to another culture and for whom Crusoe's arrival marks a profound upheaval in his life. In this imaginary dialogue, Crusoe and Friday continue this argument about identity, the problem of their initially irreconcilable identities,

and the question of the central role of history, or of the notion of time, in the representation of their cultures. Despite their differences and their limitations they attempt together to bridge the gap again which divides their respective worlds and to overcome the incompatibility of nature and culture created by modern society and modern thought. They try to find how their two identities, instead of conflicting, can complement each other. It is time to let them speak for themselves.

DIALOGUE BETWEEN ROBINSON CRUSOE AND HIS MAN FRIDAY ABOUT HISTORY AND PRIMITIVE MAN

> The true founder of civil society was the first man who, having enclosed a piece of land, thought of saying, 'This is mine', and came across people simple enough to believe him. How many crimes, wars, murders, and how much misery and horror the human race might have been spared if someone had pulled up the stakes or filled in the ditch, and cried out to his fellows: 'Beware of listening to this charlatan. You are lost if you forget that the fruits of the earth belong to all and that the earth itself belongs to no one'.
>
> (Rousseau [1753] 1994:55)

These thirty-five years away have left me worse off than any exile: I feel a sort of stranger to the whole world. When I left England it counted as one of the Great Powers, victorious after the war, but today I find an impoverished country, resigned to decline and violence in a materialistic society. Empires have collapsed, leaving no trace behind. When I came home I learned of my father's death which happened fifteen years ago. The family I knew had disappeared and, with the marriages of my sisters, a new family had grown up which I do not know. Even places have become unrecognizable, ways of speaking too, everything has utterly changed.

I have had little success with the book I planned, which I offered to the University Press. They found the text confused and too full of allusions to my own story, too full of personal remarks, which they say have no place in an academic work. In vain did I tell them that the observations made in the book were the product of the random encounters and incidents, from shipwreck to exile, which made up my story. In vain did I tell them that they depend totally on these memories; they simply refuse to listen to me. Now new theories abound to tackle this world of primitive peoples, new ideas, new approaches, of which I have never heard: I find that if I do not refer to them, by using certain words or a certain way of writing, then my work loses its value in the eyes of the 'modern' reader. I shall probably write something else, a true story this time.

'You live in a strange world, Rob,' said Friday suddenly, as I was reading the Sunday papers. 'In fact you see a strange world,' he said quietly, as if to himself.

'What do you mean, Friday?'

'I just want to point out,' he went on, 'that you seem to me to see things around you in a very strange way…How can I put it? In a lopsided way, split into two. It's very marked in you.'

'By the way, Friday, did you remember to repair the bolt on the laundry door, as I asked you last week?'

'Look,' he said, very gently, 'on the other side of the ocean, with you, I worked very hard all day long, under the blazing sun. I looked after your makeshift tools, I helped you repair your sagging cabin, I tended your sickly plants which were never meant to grow in our climate. I did all that for you willingly, for you were my guest. You sat there reading your old books, spoilt with sea-water. And now here, in your house, I still work all day long, waiting on you, and you're reading the papers this time! You still do nothing for me! That is why I say that you see things round you split down the middle, one half full, one half empty: on one side there is your world and on the other a space which you don't inhabit, and from where I come, a space in which you do not think that there could exist another world matching your own. You see it simply as a kind of marginal territory, an undeveloped area you can move into, because there's nothing there, there's never been anything there.'

Of course I could understand Friday's grievances. He seemed sunk in thought, watching the rain on the windows, as the dusk drew on. But it would be tricky to explain to him, that, although his intelligence was exceptional and he had a certain kind of wit, his status here was that of an illiterate. Friday had no education, no training which could fit him for a manual job specialized in any way at all. Besides, I thought that if I kept him on in my service it would protect him from the jibes and humiliations which he would suffer at the hands of the common people, if he had been abandoned among them with a job as a labourer or porter. Although we were so close, Friday still looked and behaved like a savage. I might add that he is fed, clothed and lodged in return for purely nominal duties, which he should not complain about.

'I'm sorry, Friday,' I said, as I folded up my paper.

'You're sorry. You're all sorry. Since I set foot in your island I've heard these words all the time. You abandon people round you to their own devices and then you are sorry, as if the society to which you belong was something separate from you, something for which you are not responsible. Let me tell you that you solve nothing by saying it's not your fault or that you feel guilty. Your finer feelings don't concern me: what matters to me, what I depend on, is the way you behave towards me. You see I'm not a fool and I know why you're sorry. You're deluding yourself, Rob: reality is not split between those things on the one hand which should happen ideally, and on the other those which happen in fact. It's your way of imagining reality which doesn't match up with reality itself and which, once again, is split in two: that's what makes you behave in such an

extremely harsh way towards those, who, like me, do not belong to your world, while at the same time feeling guilty towards them.

'That's not exactly what I mean. This burden, living under the rule of good and evil, is one of your own making. Really what I would like to talk to you about is your relationship with that world which is not your own and which you do not see.'

'You're exaggerating again, Friday, and you're mistaken. We give our full attention to the existence of other forms of culture, we gather the evidence of your traditions, we study your language and take it down in dictionaries, we collect your everyday objects and put them in museums, we catalogue your humblest buildings as we would our own historic monuments. These expeditions are long, costly and dangerous: we undertake them because we think we have something vitally important to learn from you. We devote our full attention to your world and we are genuinely interested in every detail of its organization and operation.'

'Of course. But what I'm talking about is the way in which you organize, or represent, if your prefer, your knowledge. All these details which you accumulate about our societies are gathered into a single heap which you label anthropology or ethnology. By putting them together you connect such questions as the organization of power, beliefs or knowledge. I can't fault you on that, but I want to point out that the opposite is true of the way in which you organize facts about your own society: you divide this knowledge into many different blocks, which you keep separate from one another, such as economics, political science, sociology, history, literature and so on. And yet we too work along very similar lines to all these subdivisions which you introduce when dealing with your own society. That's what I meant when I spoke just now about an uneven view, split in two.'

'Fair enough, but none the less we are forced to observe the essential boundary between subject and object: there must be on the one hand someone who asks you what you are doing, and on the other a second person who, like you, reacts and replies. Both belong really to different worlds.'

'Are you sure about that? All I see are people speaking to other people. You are trying to tell me that what makes me an object of knowledge for you is basically the fact that I am foreign, that you see me as different from you. That's not an adequate basis and you know it isn't. How could you know me if I were an alien?'

'It does become possible if I set down what I learn about you into a demonstration, by which I mean an organized proof whose truth is controlled by logic. Thanks to that, I can observe patterns or irregularities in what you say or do and then I can make hypotheses and match them up again with the facts, with what you are, and adjust my interpretation accordingly. So you see that, thanks to this constant movement to and fro between the world of things or facts and the world of ideas I can deduce general principles about reality and thus work out knowledge from what I do not know initially. The other advantage is the

existence of a methodology which sets out all the stages by which you produce the knowledge. This means that anyone, provided he follows the method, can produce the knowledge and check what has already been produced. You too, Friday could work out your own ethnology or your own archaeology'.

'Wait a moment. This is surely the core of the question. There seems to me to be some confusion in what you have just said which is peculiar to you. You are conflating a universal system, call it scientific proof, if you will, with a single situation, I mean your own situation, your own story. But they are two totally different things. You are confusing them because you are genuinely convinced that your own world is the only possible one, in other words, that it is universal; in your eyes the history of your own world is merged with the history of the whole world. It follows naturally that you divide foreign worlds into categories which refer to your own history, or rather to the idea you have of your own history. First you start with primitive peoples who live in a basic state, barely above that of animals, then you move on to barbarian or underdeveloped races who are credited by you with the invention of the first forms of culture, and lastly you come to the world of nations or states which you think of as modern, that is to say as belonging to civilization. It is very revealing that you seek the more advanced or civilized societies than yours in the most unlikely place they could be; not on Earth, but in the stars. I find it very significant too that you find it necessary to help those intermediate peoples, which I have just mentioned, by developing there what you see as the momentum of progress towards civilization, agriculture and industry. As for us, the primitives, we attract and repel you at the same time, since you see us as belonging to a sort of original state, in which all the urges which are curbed in your society, can be expressed freely. You dream of returning to this lost Paradise, where everything would be simple and pure, but you are scared of your bestial instincts being set free. All this history is just a dream to you, Robinson—the primitive races in the primordial forests, the birth of the towns and the expansion of the great empires on the Earth's surface, the spirituality of future civilizations which people the terrifying void of the night sky.

'It's not so much your own history that you superimpose on the history of the whole world as the image you have of this history in its guiding principle: you think that history unfolds by *itself* from nature towards culture, from the primitive to the civilized, the rudimentary to the sophisticated. This is why you find us so fascinating: we actually represent something *pre-historic,* something from before the dawn of history, before the start of this process which leads to civilization and the loss of the state of childhood. We make you feel that time has been abolished, by transporting you in your imagination to that era of origins, which you believe can reveal the meaning of your identity. This also explains why the idea of time or history is basically incompatible with what you call anthropology and why, in your mind, there is an obvious and almost natural connection between anthropology and prehistory. Your ethno-archaeology is simply the expression of this imaginary link, and the very idea of an

ethnoarchaeology of the society in which you live is totally unthinkable, precisely because this society is part of history and belongs to modernity.

'If, as you suggest, I were to set down my own pre-history or my own ethnology, that would mean basing my argument on this idea that you have of world history or rather, if you prefer, adopting for myself the concept that you have of your own identity. In other words that means I must accept as true the confusion I mentioned just now, this amalgam of the particular and the general, and give up my own identity. By offering to act in your version of the myth of origins I justify the false role that you want me to play, the one which actually obeys the true rules of your moral system. You turn me into an apprentice who is restricted in what he can do, a pupil who follows your teaching and whose slightest whims are channelled into a speech that you have prepared in advance for me. You yourself appear to full advantage, tolerant, kindly, concerned about the free expression of differences, which you dismiss as belonging to a *minority*. If I agree to this deal, I submit to your authority and appease your conscience at the same time: only fools or hypocrites can profit by this distortion of the truth, admitting such a *politically correct* falsification, which consists in twisting the meaning of words. I'll tell you one more time: you are putting yourself at risk by agreeing to yield to the contortions imposed on you by a system of morality in which you have to appear not to be doing what you are doing. You are enslaving yourself by submitting your thinking to a justification of social inequality.'

'Let me just interrupt a minute. I agree with what you say about my imposing the idea I have of my own history on the history of those who are different from me, but what else can I do? I can't get interested in history in which I'm not involved. I'd even go as far as to say that I can't conceive what it would consist of, if it did exist.'

'That's what I mean when I talk about this 'idea you have of history'. I'm referring to what you define as the principle which would characterize history, and would be recognized as an absolutely distinctive feature.'

'You mean historicity.'

'If you like. What I mean is that you define this principle of history above all as a process of transformation which has its own features, like an engine or a clock mechanism. Your history rolls on from primitive tribes to stratified societies, and then to civilizations and states, at a particular rate of flow which corresponds to something deeper: this course denotes not only the irresistible transformation of nature into culture, but also the intrinsic movement which leads from primitive organisms to complex structures. To put it another way, if I turn over this principle of history, like a pebble on a river bed, I perceive the categories according to which your knowledge is organized. I expose this opposition between nature and culture and I throw into relief this essential point, the notion of time: I mean the representation of time as evidence of systems changing, a sort of description of their identity'

'I'm afraid I don't quite follow you.'

'Let me give you an example. Suppose you could have access, from its very beginnings, to the archaeology of one of the primitive races who live naked in the forest and use stone tools, and see what elements are available for you to study: those types of flora and fauna more common at this period, those types of tools or activities more frequent at this time. But basically there is no difference between the stone knife you see crafted before your very eyes and the one you find buried deep in the earth, almost absorbed into the rock. Tell me now how you are going to write that particular history'

'I don't know and I don't think I can.'

'That's right. You can't, because here you're dealing with materials which show variations, not transformation, and in this instance, your principle of history cannot be applied. History becomes possible once more when *you* arrive, when you cut down the forest and build roads, when you put in settlers, build towns and open mines. Because you introduce a time or rather a temporality which is your own and which is also that of history. Now we're getting to this very weird idea that history, as it's produced, as it happens in reality, and history as it's represented by you are very closely linked and are defined in relation one to the other. Before you, there was neither time nor history, but only pre-history over which you really had no control. Or else, you must imagine a different temporality, which takes into account both your identity and what's different from you; that's to say that you must take on a new identity. Here notions of temporality, form and identity are all bound up.'

'But I can measure this temporality you talk about. I can measure it, date every moment of the past, and so I can situate historical events in relation to one another. I can reassemble the elements of a history whose direction is clearly irreversible. I can isolate processes which exist in themselves.'

'Of course you can do all that, but once again I want to talk to you about the meaning attached to this temporality, or, more precisely about the idea of chronology as the expression of a change introduced by time. In your representation of chronology the first characteristic of the flow of time is not its irreversibility but rather the fact that this flow is cumulative, that it produces an accumulation of matter transformed by time, that is to say, history.

'Look! This principle of chronology is the one shown by an hourglass or an antique water-clock, invented precisely to show the flow of time. If I turn upside down this hourglass which the cook uses to time hard-boiled eggs, I set off a transformation process which will accumulate the sand at the bottom of the glass until the little chamber above is empty, until there is not a single grain of sand left to come through. Inside your hourglass each moment is distinct from the one which went before, in the sense that it introduces something new which will transform, by bringing in a new amount of grains of sand, the sum of all the preceding moments, the little cone of fine powder which builds up gradually on the floor of the glass.

'Now if you look more closely you will see that inside this continuous transformation each moment is linked to the previous one and to the following

one by a series of effects derived from each other, which you can take as a chain of *causalities:* each grain of sand which comes through and rolls onto the surface of the cone finds its place as a result of the position of the grains which went before it into the bottom of the glass. I do like this idea of your hourglass in the kitchen, because it's a closed system, totally artificial, cut off from the outside world by a transparent sheet of glass so that one can see what's happening in the inside. Since it's an instrument cut off from what surrounds it, this means too that in the way in which it keeps track of transformation by time it cannot take in anything new—only grains of sand can flow inside your hourglass. But you are aware that what feeds the course of history is precisely the incessant flow of new things happening. This implies that your system is *continuous* from start to finish. It can *evolve,* but not change radically, for you always obtain a little cone of sand at the bottom of the glass and the transformation stops when the matter is exhausted. Now once again this representation of temporality does not correspond to the reality in which we live, you and I, because, on the contrary, the matter which feeds history is being constantly renewed.

'All this raises deep questions to which we have no answers. If the sequence of causes is not sufficient to explain history, as your hourglass is, what are we dealing with here? And how do the forms which we see manage to keep their shape, to keep their identity continuously in time? But this discussion would take us too far.'

'I see what you're leading up to, Friday,' I said. 'You're questioning the idea of linear time, the idea of time which carries its own progress within itself, the idea of a world which works like a clock, whose regular ticking can be heard from one side of the universe to the other. That's fine. That's what the physicists have been saying ever since Mr Newton's discoveries, when they show us that the difference between past, present and future is merely an illusion. I agree with them, but you see, that sort of time is not our sort of time, it's not the time of history, which is what we have been discussing nor the time of literature or narrative, in which we are now.'

'Exactly so, and that's because we define what we mean by identity by referring to time. That's also why the question of time or of the representation of time is of crucial importance for you, so that you can discover your origins, about which you really know nothing, and for me too, so that I can recognise an identity which is not dependent on yours.'

'Where are we to find the points of reference then? This problem of prehistory you bring up means mastering deep time, converting the vastness of immemorial time before history into something human. A mighty abyss now yawns at our feet since we discovered that a succession of unknown worlds preceded our own, and we are trying to fill this abyss of time with whatever we can. You're surely right to say that we're attempting to stop this gap in our identity, this chasm in our past, opening onto emptiness, with history, with materials made out of our idea of history. I would go further than you in this respect. It's not just history that we insert into prehistory, it's literature too, in fact it's literature above all.

For history and literature share through narrative the same understanding of time. We try to transform this empty space of our origins by using a narrative or a story which applies to us. It's through characters, settings, actions that we try to fill this absence of history, this gap which precedes stories. This can be the only plausible justification that I can give you for these borrowings from ethnology or ethno-archaeology, that we were discussing just now, I mean filling in the narrative frame of this history which still remains to be written, 'helping us to imagine', in brief, supplying material for what we must call romantic borrowings. But Friday, what else can we possibly do? If you and I are creatures of fiction, it's because our story forces us to exist. Tell me if you can: what else could we have done?'

'You said it just now, Robinson. You can't reconcile this deep time which is revealed by carved stones and fossils, this time in which you didn't exist with this time of history or stories which provides the dimension of your identity. Listen: the world keeps changing and with it the way we represent things around us. Scarcely have your ships landed in every country and your goods begun to spread out to the whole of humankind, than already the security of beliefs which makes this world system plausible starts to disintegrate. You must, we both must, imagine something else, that's all.

'This kind of time, flying like an arrow straight towards the future, overtaking each past moment by driving on always higher beyond known limits, this time already belongs to a world which has been overtaken. If you express the passing of time as a causal sequence you are putting forward the hypothesis that the world is transformed exclusively by actions and that it is possible to influence its course. Once again you are contrasting passive nature, destined to be transformed, with active culture, destined to tame nature. You have to change this way of seeing things. It's no longer adequate to describe the world around you and it's become dangerous for you and me. This way of portraying time as a kind of constant overtaking, thanks to the never-ending insertion of the new, this is what forces us to see the renewal of knowledge as a series of revelations or revolutions. Again and again it makes the earlier constructions, carefully elaborated over a number of years, fit only for the scrap heap. These breaks deprive the evolution of knowledge of the continuity it needs to establish itself properly, and they split up our body of knowledge into different sections which become ever more specialized, harder to join together. Here we are back at the start of our discussion, when I mentioned how fragmented your knowledge was, and how your view of the world was not well balanced, and you, for your part, raised the difficulties you had in transcribing what you had learnt from your travels in a style acceptable to those who control the production of knowledge in your country'

'Since I came home I keep feeling a sense of emptiness and dislocation. More and more I feel that the way we describe things does not correspond to the way in which they behave, that they move more easily than we imagine from one category to another, or else that they are revealed at the point where several

dimensions intersect, while we see only one or two. I don't know what happened to me all that time I was far away with you. I'm not sure if I have gone half native myself or whether I have simply missed the next stage in the story which was going on here without me. Whatever happened, I have lost my bearings. Now that I've made myself an outsider to my own society but haven't quite taken on another identity, I am afraid of being reduced to a mere wanderer, always changing places and never having anywhere to come back to again. I can see what you mean, Friday, I can follow what you're saying, but the fact that I can understand you is partly because I no longer belong properly to the world I came from originally. I have been transformed, my friend, but the transformation is incomplete, and I am torn by conflicting emotions: I want to set off again and I want to stay here. I can see that we are going to devour the world with our ships and that, once that is done and there is no corner left, we will asphyxiate ourselves. I can see that the world is shrinking, that the horizons of our imagination are closing in, but I don't know how to escape from this confinement.'

'This world is over, Robinson, and we must imagine something as yet inconceivable. We must define our identity using new guide marks, which will not imply emptiness for you nor extinction for us. We must found it on a new representation of time. A time which is less crude than that of your clocks, a more flexible time in which past and present are no longer opposed, but supplement each other, constantly renew each other in a movement which begins over and over again but is never twice the same. Imagine time no longer as an arrow nor an abstract line, but as a wave on the surface of the water, Robinson. Or else as a river: a river springing from a single source, but swollen by a host of other rivers whose waters flow into it, blocked by hills, but carving out its bed in the mountain rock, puny and faltering as a stream, but mighty and unstoppable when it pours into an estuary by the sea. Time twisting and turning on itself like a current, ceaselessly mixing the present which forms on the surface and the past which comes up from the depths. You would need a more sensitive instrument than your hourglass, however accurate it may be, in order to measure that sort of time, and to see how past and present, instead of drifting indefinitely away from each other, fundamentally keep on supplementing each other' (see Fig. 18.1).

'That means there must be new sorts of history, archaeology, anthropology. It implies a knowledge of humans which is no longer defined by referring to their difference or their special features, or a knowledge of the past which does not depend on reference to the specificity of the past. Both are impossible to characterize in themselves, since this particular feature of humans is defined with reference to other humans observing them, and this specificity of the past is conceived with reference to the present. One would have to imagine a knowledge of the past which could accept the transformations of the past and the contributions of the present not as a succession of fragments or a series of states which could not be compared with each other, but rather as dimensions within

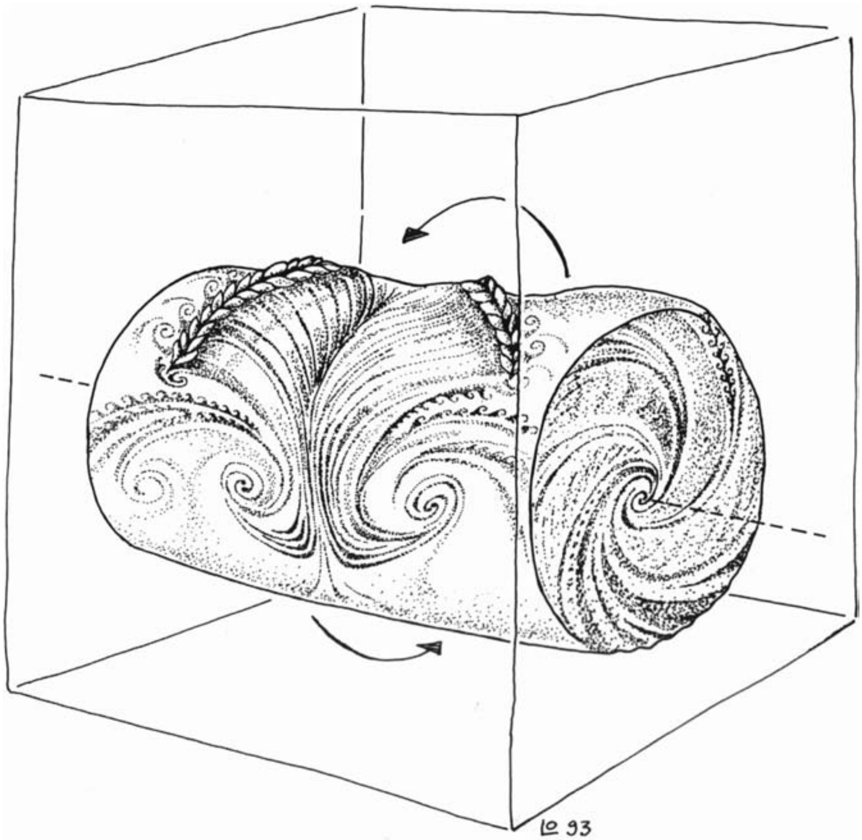


Figure 18.1 Man Friday's time observing machine

which the forms of the same identity would develop and recreate themselves continually. I think I'm beginning to see more clearly, Friday.'

'Let's go out for a drink now. It has stopped raining. And just do me a favour, would you?'

'What's that, my dear Friday?'

'Stop calling me Friday. I remember that Friday long ago when we met, but you forget that it was I who found you on the Ile de la Cité, where you were living rough on the banks of the Seine like a tramp, stinking and ragged. It was I who made you take a bath. Come on, get your hat and your money! We can carry on solving the world's problems, but this time with a different tool: a good bottle of wine.'

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